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KNOWLEDGE AND THINKING *)

BY

L. SZÉKELY, *Stockholm*

> "The ability to foresee and to plan ahead, that is the ability to use past experiences in order to establish future conditions corresponding to the needs, desires and the aspirations of man, is the first essential prerequisite of freedom".
>
> B. Malinowski, "Freedom and civilisation".

INTRODUCTION.

An examination question may serve as the best introduction to the problem of our investigation.

At the juridical faculty of B. University the candidates are tested with respect to their knowledge of the Penal Code. Under discussion is a rule of law stating that "Whoever damages or destroys for revenge another person's property is guilty of a crime....", whereupon punitive measures are stated.

The examiner then asks the first candidate: "Suppose you were the judge and had to give a decision on the following practical case. Somebody has been accused of having wilfully, for revenge, thrown someone else's ring into a river....". The candidate: "I would find him guilty". Examiner: "But the ring was not damaged or destroyed. If a diver brings it to the surface, it will be exactly as it was before. — What about you, candidate B."? — "I would acquit him". — "Why"? — "Because the ring is undamaged". — "Well, in that case any person could take revenge by throwing other people's property into a river. — Should he get away with that"? —

Six or seven other candidates are unable to find their way out

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*This research was performed 1944—'45 while the author was archiv-assistant on the Institute for Psychology and Pedagogics, University of Stockholm, (Director: Prof. D. Katz).

For translating into English I am indebted to Dr. R. Palm, New York.

of this legal dilemma. Finally someone says: "I would find the defendant guilty". — "Why"? — "It is true that the ring as a physical object is undamaged. But the ring has also a value. It is a valuable that one can pawn, sell, etc. Lying at the bottom of the river, however, the ring has no value. It regains its value only after the diver has brought it back to the surface. That means that its value was restored through human labor. Labor for its part, has its specific price. In the end, the cost of this labor was equivalent to the actual damage". — This answer was accepted.

In what way is the solution of the problem attained? The ring is considered from a new point of view. It is not just a physical object, it is also an object of value. The criminalistic fact of the damage or destruction is shifted from the physical aspect of the object to the aspect of its value. The "concept" ring is restructured, reorganized.

Is it really such an unusual idea that the ring represents an object of value? Not at all. On the contrary, that aspect of a ring which usually presents itself foremost to our mind, is that of its value. To the question of what they would give for a ring laying at the bottom of a river, all candidates undoubtedly would have replied, "Nothing. There it has no value". What is then, in fact, so original, new and unusual about the idea that led to the solution of the problem?

In the process of thinking normally those qualities of the object impress us which are characteristic of its common use (R. Katz 6). We approach a ring with tendencies like "have", "acquire", "sell", etc. and these tendencies usually provoke the question, "What is its value"? Our juridical examination case, however, refers to destructive activities. Here the crucial aspect under consideration is the destruction or damaging of the physical nature of the object, and the candidates constantly view the problem from this angle.

Can we assume that the difference in the reaction of the candidates, their success or failure in solving the problem, stems from individual differences in their knowledge? Very unlikely. They all were equally familiar with the wording of the law and the fact that the ring has value, was no secret to any of them. The difference is to be explained by the fact that in the mind of the one candidate a certain quality which was not brought

out by the situation as such, became "thematic", while in the mind of the others only the habitual aspects of this situation were functionally operative.

Our examination case is typical for those situations in which we are confronted with the problem of the relation between knowledge and thinking. To solve a problem, a certain knowledge is required. But out of a group of persons with the same, and for the particular problem sufficient knowledge, only relatively few are able to use their knowledge effectively and productively.

Experiences of every-day life often have affected the handling of scientific problems. Experiences of this kind led to certain, not very clearly defined, theories about the temporal localisation of productive processes. According to these theories, all the mental processes involved in productive thinking would take place only during the act of problem solving itself. Whether or not a solution of the problem is attained, would depend exclusively on the present solution process and would be independent from the characteristics of all the previous processes through which the present knowledge and experience was acquired.

It is the aim of the following investigation to examine the correctness of this assumption.

Relation between knowledge and thinking

The functional relation between learning (acquiring of experience), knowing and thinking can be approached from three different angles.

Knowledge is first of all the end-result of learning, of experiencing or, in rarer cases, of independent thinking and reflecting.

Secondly, knowledge can be the starting point for the absorption and the understanding of something new, or for thought continuation (thought progress). — Our attitude towards a present event f.i. is dependent on our past experiences. It depends on our past experiences whether we put the present one as self-evident "ad acta", or whether we are surprised about it, try to find an explanation for it, are stimulated to further thinking, etc. (Székely 13).

Thirdly, our stock of knowledge serves as the matter and as

the aid which one applies in the thought process, in the solving of a present problem and in coming to terms with a new situation.

One can therefore investigate knowledge in its function of 1) end-result, 2) starting point and 3) medium of learning and thinking.

In the field of memory psychology a great deal of attention has been given to the problem of knowledge as the end-result of learning and experiencing. Also knowledge as the end-result of thinking has been studied by a number of authors. In this connection the studies about theory-formation in children by R. Katz (6), Piaget (9), Huang (3), E. Becker (2), C. Raspe (10) should be mentioned. As is known, all these studies have been conducted mainly by the method of questioning.

Knowledge as starting point of independent thinking has not as yet been investigated experimentally. The studies of Székely (11, 13, 16) represent the first beginning in this direction. Knowledge as starting point of independent thinking might be investigated through certain experiments in which the subject has to forethink what might happen in a certain, never before experienced situation. In that way it is possible to observe whether and how the subject applies a certain piece of knowledge accessible to his memory; also whether and how a certain piece of knowledge which is no longer accessible to the "representative" memory, (that is: "forgotten" knowledge), can nevertheless be used.

For the investigation of knowledge as medium ("vehicle") of thinking, certain problem-solving tasks are instrumental.

In order to examine the methodological value of these different possibilities and especially in order to clarify the theoretical situation, we have — in a few preliminary experiments-presented the same thought problem from all three angles. In the first experiment the subject is confronted with a phenomenon which is new to him, in order to obtain (post factum) his explanation of what happened. In the second experiment the same phenomenon has to be forethought, before it manifests itself (ante factum). In a third experiment the same phenomenon has to be experimented upon freely by the subject in order to find the solution. Of course different subjects have to be used for each of the three experiments.

PRELIMINARY EXPERIMENTS

1. *To explain a new phenomenon after observing it (post factum).*

A candle is placed on one end of a quadrangular lever, about forty centimeters long, and the lever is balanced on the beveled edge of a triangular prism (Fig. 1). Then the candle is lighted.

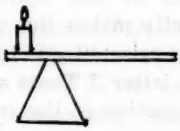

Fig. 1. Equilibrium Task

After a few minutes the lever tips over. The subject now has to explain, why the lever fell down.

2. *To think out an event in advance (ante factum).*

The same arrangement, but without lighting the candle. The subject is asked: "What would happen, if the candle were lighted"?

Experiments showed, that it is more difficult to foresee an event, than to explain it after it has happened. Small children cannot anticipate the tipping-over of the lever, but are able to give an explanation post factum. The ability to foresee, to forethink, is generally the more important social function. Anybody in every-day life may find himself in situations where he has to make a decision in favour of one or the other way of acting and his choice will depend on his ability to forethink.

3. *To find a solution through free experimentation.*

A lever, a prism and a number of small objects, among them a candle and a box of matches, are placed on the table in front of the subject (Székely 11). The subject is supposed to balance the lever, after burdening it on one end with an appropriate object, on the edge of the prism, but in such a way that it tips over automatically, i.e. without being touched, after a few minutes.

In this form the problem is the most difficult and its solution requires, in the vast majority of cases, several minutes and considerable mental effort.

Discussion of the results

We confine ourselves to a qualitative evaluation of the results. The essential piece of knowledge, the application of which leads to the solution of this problem in all three situations, is the fact, that through burning the candle loses in weight. This fact is, it would seem, common knowledge. Why then is it so difficult for a considerable number of the subjects to dispose of this knowledge? What exactly makes this problem such a difficult one? How does the idea arise: "Light the candle, place it on the lever and balance the latter"? These are the questions which interest us. The introspection of the subject, however, fails us entirely.

It is the investigators and not the subject's task to describe the process involved. He has to form a hypothesis and then test it through appropriate variation of the experimental conditions. It is our hypothesis, that in the common notion of the average present-day adult solid physical bodies, amongst them the candle, have a constant weight. Weight, however, is by no means a relevant or characteristic quality of the candle. Loss in weight through burning is not an essential part of the conceptual structure "candle". We rarely observe this quality of the candle in its every-day use, as we hardly ever have to react to it. It is, in psychological terms, a latent quality of the thought object. The solution of the problem is attained through the discovery of this latent quality, i.e. through restructuration of the concept "candle". This hypothesis cannot be verified or disproven through introspection of the subject, but only through control experiments.

The problem task is now being altered in such a way, that among the various objects offered for choice, there is a small bottle with the inscription "ether" or "benzine". Ether and benzine are volatile substances. Their volatility is not a latent, but a manifest quality, highly relevant for their practical use, a quality to which we are accustomed to react. While in the case of the candle the difficulty consisted in the fact, that a latent quality had to be discovered, i.e. a piece of knowledge had to be reorganized, this difficulty does not exist in the case of ether of benzine.

This was confirmed by our experiments. In the latter form

the problem task is so easy that even malaria-treated paralytics, who otherwise came to be entirely incapable of productive thinking, were able to solve the problem promptly. When they, however, were told that there was still another way of solving the problem whereby not the phial, but another object had to be used, the patient failed (Székely 11).

We now have gained our first insight into the functional relation between knowledge and thinking:

1. There is what one might call an "initial knowledge" (f.i. that "solid bodies have a constant weight") which phenomenologically does not reveal itself in the process of thinking, but which is nevertheless functionally operative. The different propositions for the problem solution are dependent on the initial knowledge — and for that reason may or may not lead to the goal. The substance of this initial knowledge and its functional operation are disclosed through appropriate variation of the experimental conditions. The introspection af the subject does not render any information about it.
2. In the process of productive thinking a piece of knowledge is reorganized.
3. The reorganization of the initial knowledge takes place on a thought object (f.i. the candle) in which the initial knowledge is so to speak "embodied". The problem solution is dependent on the outcome of this reorganization.
4. The vicissitudes of this initial knowledge, its transformation, its reorganization in the process of productive thinking, all this does not reveal itself phenomenologically. Only the result of this reorganization emerges as "idea" or as "aha-experience". Explanation about the fate of knowledge in the course of the thought process can thus be found only by way of systematic experiments and not through introspection.

There is, in principle, no difference whether the subject searches for a post-factum explanation, forethinks or devises means to reach the goal. The subject starts out with an initial knowledge that is being reorganized in the process of thinking; he starts out from a thought object that is being enriched with new qualities, and in the end he leaves the problem situation with a modified knowledge. The three situations differ from each other in the degree of difficulty with which the restructur-

ation is achieved. This leads our attention to the fact, that the restructuration of a piece of knowledge not only depends on this knowledge by itself, but also on the total situation (Székely 12, 15). In the present study however, we shall not go any further into this problem.

The preliminary experiments have to some extent clarified the relation between knowledge and thinking. The next step will be to investigate the

FUNCTIONAL RELATION BETWEEN LEARNING, KNOWLEDGE AND THINKING.

For this purpose we have chosen a problem task that requires an initial knowledge which itself is a result (or residue) of previous learning. The knowledge of the Hydrostatic Principle (Archimedes' Theorem) proved suitable for experimental purposes. The subjects were given a thought problem, the solution of which presupposed their familiarity with this physical law.

Experimental series I

Inclined Plane Task (Székely 13)

In the middle of an inclined plane stands a small carriage, attached to a string. This string leads around a pulley which is fastened at the upper end of the plane. On the opposite end of the string a metal weight is attached. Under this weight is a glass container, half filled with water, in which the weight is partially immersed. Carriage and weight are balanced, the carriage thus being at rest. Besides this, there is in the table another half-filled water container and a glass pipette (Fig. 2).

Fig. 2. Inclined Plane Task

Experimental procedure

The subject is asked the following question: "How can you, by means of the pipette, set the carriage in motion and make it move upwards for about 1—2 centimeters? You are not permitted

to push it or touch it in any way. Think how you could solve this problem".

After this problem is solved — or after the subject has failed — the metal weight is replaced by a piece of wood. The latter also partially sinks into the water and thus the carriage is brougt to a balance again so that it stays at rest in its initial position. At this point the subject is again asked the same question.

This experiment is followed by a series of other thought problems, which equally imply familiarity with the Hydrostatic Principle as initial knowledge. These experiments will be discussed in the following. At the end the subjects were tested orally in respect to their knowledge of the Hydrostatic Principle.

Thirty-eight subjects, graduate and undergraduate students of Stockholm University submitted to the experiments.

Results

First a few typical protocols.

Protocol 1. Subject deliberates silently ... "... I am not at all trained in physics ... would nevertheless think it possible ... (long, silent deliberation) ... that ... one ... has to take out some water ... (hesitating) ... (Experimenter: Why?) ... Should the carriage move upwards? .. (Experimenter: Yes.) ... Aha! ... I imagine the weight should then sink down ... no, just a moment ... but this is not possible ... if there were no water ... one has to add more water ... (Experimenter: Why?) ... Because, in my opinion more water would exert heavier pressure on the metal weight and thereby make it sink deeper". (Subject is uncertain, has no clear picture of what would happen).

Experiment with the wooden weight:

Subject: "This weight is of wood ... Aha!!! Now it is clear to me! Take water out. If water were added, the weight would rise, would float upwards ... so one has to take water out". (Subject is entirely certain).

Protocol 2. "When a body is immersed in water, its loss in weight equals the weight of the displaced quantity of water ... but in this case I am very uncertain ... the weight hangs on the string ... I don't know which is the decisive factor, the weight on

the string or the loss in weight per gram of water? . . .well, being in water one is lighter than after one comes out of it . . . Here is a case of analogy. To make the load heavier, the factor of water, which causes the corresponding loss of weight, has to be eliminated".

Experiment with the wooden weight:

"Here the principle is the same, even though the wood is lighter". After this the subject remarks: "Now the reasoning is much easier".

Protocol 3. "Take water out. Every body loses weight in water. So if there is no water, or less water, then it will sink. — And in the case of the wooden weight?" — "The same way".

Many of the subjects were puzzled by the metal weight arrangement. "Could adding or taking out of water cause the weight to move"??! — "It would have to get heavier; but does it get heavier by adding or removing water"? — The weight hangs on the string; what can the water have to do with it"? — "The weight does not change, even if one adds more water" etc.

With the wooden weight all subjects understood immediately that the water level had to be lowered in order to make the weight sink and to pull the carriage upwards. In the case of the metal weight arrangement, however, it was to some of the subjects not at all, or at least not immediately, clear what had to be done and how these changes were causally connected. Some of them succeeded in solving the first task (metal) only after the second experiment (wood). It suddenly struck them, that the water level had "naturally" to be lowered and that there was no fundamental difference between wood and metal. Others, however, were of the opinion, that with the metal weight arrangement, one had to proceed in a way opposite to the one with the wooden weight, because "metal sinks, wood floats".

To many of the subjects the arrangement with the wooden weight was "more clearly understandable" than the one with the metal weight. When the metal was replaced by the wood, the sudden change in the intellegibility, the perspicuity of the situation was obvious. Why, in fact, is one situation so much easier to understand than the other?

As long as the metal weight hangs on the string and is

partially immersed in the water, there is — in the mind of the subjects — no connection between the spatial position of the weight and the height of the water level. A little bit more or less of water does not alter the position of the weight. For that reason the subjects actually have no idea as to what to do about the water. But as soon as they deal with the wooden weight, they are at once aware, that its position is definitely determined by the water level. Wood floats. Metal does not float, but sinks in water; and if, as in our case, it does not sink, then only, because all its weight is carried by the string to which it is attached. In the case of the metal weight, the water level is — in the opinion of the subjects — an element of the experimental arrangement which can be changed at will, without at the same time affecting the other elements (position of the metal weight and of the carriage). With the wooden weight, however, any change of the water level brings about a change in the other elements (position of the wooden weight and of the carriage).

From the way they approach these two tasks one can draw conclusions in regard to the "initial knowledge" functionally operative in the mind of the subjects. They approach the solution of the problem with the naive empirical knowledge of every-day life. According to this conception — "wood floats, metal sinks" — floating and sinking are qualities of the objects and opposite ones at that. Those subjects to whom the metal weight arrangement was clear from the beginning, approached the solution with a different initial knowledge: with the knowledge of the Hydrostatic Principle acquired through the study of physics. According to this principle floating and sinking are not qualities of the bodies but functions of gravity. Whether a body floats or sinks depends on the ratio between its weight and the weight of the displaced quantity of water. Floating and sinking are not opposites; both are governed by the same principle. — There were finally a few subjects who only after longer deliberations, in some cases only after finding the solution of the wood arrangement, began to understand the arrangement with the metal weight. With these subjects the initial knowledge changed in the process of problem solving.

All subjects had at one time learned the Hydrostatic Principle. So it may safely be assumed, that those subjects in whom this knowledge was inoperative (i.e. did not function as initial

knowledge), had forgotten what they had learned. The examination of the memorized knowledge showed, that eighteen subjects could recall the Hydrostatic Principle, that is: could adequately reproduce its essence. Twenty subjects were not able to recall it or to reproduce its essential meaning. The first category of subjects we will call "subjects with adequate memorized knowledge", the second one "subjects without adequate memorized knowledge". Table I indicates the solution frequency with the metal weight arrangement for the two groups.

This table shows, that all subjects with adequate memorized knowledge could solve the Inclined Plane Task with the metal

TABLE 1
Initial knowledge functionally operative in the solution of the Incined Plane Task (metal weight).

	Knowledge of the Hydrostatic Principle	Naive empirical knowledge	Hesitancy
S. with adequate memorized knowledge N : 18	17	1	0
S. without adequate memorized knowledge N : 20	8	9	3

$r_B = 0{,}57\ \varepsilon_r = 0 = 0{,}16$

TABLE 2
Initial knowledge functionally operative in the solution of:

	Hydrost. Lever Task II metal weight			Hydrost. Lever Task III unsymmetrical		
	Knowl. of Hydrost. Principle	Naive empir. knowl.	Hesit.	Knowl. of Hydrost. Principle	Naive empir. knowl.	Hesit.
S. with adequate memorized knowledge N : 32	17	13	2	4	15	18
S. without adequate memorized knowledge N : 18	9	8	1	1	16	1

weight. Some of them, however, were hesitant and arrived at a clear understanding only after having solved the task with the wooden weight. Yet in earlier experiments with the same task (Székely 13, 15) we found a number of subjects who, notwithstanding their adequate memorized knowledge, were not able to solve the metal weight task. On the other hand eight of the subjects without adequate memorized knowledge nevertheless solved the metal weight task right away. Thus a piece of knowledge that was no more accessible to their representative memory, was — strangely enough — functionally operative and could in a problem situation perform the function of the initial knowledge. How is this to be explained?

In earlier studies (Székely 13, 15) the following hypothesis about "understanding" was formulated: When the subjects were taught the Hydrostatic Principle, they had already some idea about hydrostatics. They knew f.i. that "wood floats, metal sinks" etc., but they did not know the law behind it. Their knowledge, before having learned the Hydrostatic Principle, is equivalent with what we previously defined as "naive empirical knowledge". With the intake of the new learning material this naive empirical knowledge functions as initial knowledge. The vicissitudes of this initial knowledge in the course of the thought process — whether it is transformed or remains unchanged — is essential for the psychological assimilation ("understanding") of the new learning material.

The "naive empirical knowledge" seems to be incompatible with the knowledge of the Hydrostatic Principle. According to the first, the physical bodies would fall into two groups with opposite qualities: those which swim and those which sink in water. After understanding the Hydrostatic Principle a person gains a different conception of the physical world. Swimming and sinking bodies now do not constitute two groups with opposite qualities, but are subject to the same physical law. Thus the "naive empirical knowledge" is transformed, reorganized. The "understanding", "comprehending" of the learning material and this process of reorganization are correlated.

Where this process evolves, meaningful learning takes place. Thus the knowledge of the Hydrostatic Principle may in a later problem situation (like f.i. the Inclined Plane Task) assume the function of the initial knowledge. If, however, this reorganisation

process does not take place, the subject has not comprehended the learning material. When later-on a thought problem has to be solved, then only the naive empirical knowledge, which remained unchanged, can be functionally operative.

In the course of years the verbal contents of the learning material may be forgotten: yet this would be of no consequence. No matter whether or not the verbal contents remain accessible to the representative memory, subsequent problem solving is determined by the previous assimilation of the learning material, which in turn made use of the already existent empirical knowledge.

This hypothesis of meaningful learning contains two theorems:

1. *Theorem of the analogy or correlation between meaningful learning and productive thinking.* There is in the beginning a certain initial knowledge present or functionally operative which in both processes is changed, reorganized. Thus both these processes result in the transformation of a piece of knowledge.

2. *Theorem of the functional continuity between meaningful learning and productive thinking.* It depends on the proper assimilation of the learning material whether or not it can be used later-on in a problem solving process. Only the knowledge (experience) that was really absorbed, assimilated effectively, can serve as material for a new productive process. The initial knowledge with which, in a certain problem situation, the solving process starts out, is in its turn dependent on the characteristic elements of an earlier learning process.

Katona (4) proved already, that "understanding" goes hand in hand with a certain organization of the absorbed learning material. Our first theorem, however, goes one step further. While Katona refers only to the organization of the newly absorbed learning material, our theorem maintains, that in addition to the assimilated new material also old learning material is included into and transformed in the process.

But our two theorems may perhaps not be necessary for the explanation of our findings, gained through the Inclined Plane Task. One can make the opposite assumption, that the functionally operative initial knowledge is not dependent on the characteristic elements of a previous learning process, but on the structure of the present problem situation.

Let us now examine this assumption and to this end alter the problem task. The new thought problem consists, like the Inclined Plane Task, of two parts. One part can be solved only through knowledge of the Hydrostatic Principle, whereas the second part can be solved on the basis this knowledge as well as on the basis of the naive empirical knowledge.

EXPERIMENTAL SERIES II

This series of experiments consists of two problems: Hydrostatic Lever Tasks II and III.

DESCRIPTION OF EXPERIMENTAL ARRANGEMENTS AND PROCEDURES:

1. *Hydrostatic Lever Task II*

The arrangement consists of a long, narrow board (50 × 10 centimeters) which serves as a see-saw. In the presence of the subject this board is burdened on one end with a half-filled water container, and then balanced on the narrow beveled edge of a prism (Fig. 3). Over the container a metal weight is

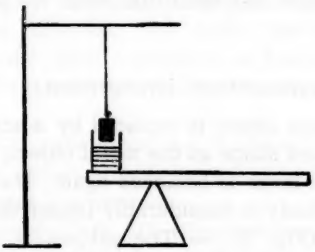

Fig. 3. Hydrostatic Lever Task II

suspended, on a string which is fastened to a pole. The subject is asked the following question: "Imagine that this metal object were lowered, until it is about halfway immersed in the water. The object does not touch the bottom of the container, nor does it anywhere toch its walls. "Will something happen, and if so — what"? After the subject has expressed his ideas, doubts etc. or has left the question undecided, the metal object is replaced by a wooden object of approximately the same size, and the question is repeated.

2. A. *Hydrostatic Lever Task III (Symmetrical Arrangement).*

The arrangement consists of a quadrangular lever, about 50 centimeters long, which can be balanced on the top of a pole. From each end of the lever a body is suspended: at the left a large wooden object, at the right a small metal one, both of the same weight. Thus, when in balace, both arms of the lever are of equal length (Fig. 4). Underneath is a large container. — The

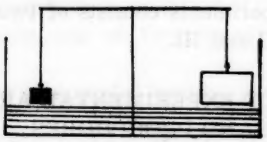

Fig. 4. Hydrostatic Lever Task III Symmetrical

following question is put before the subject: "Suppose we filled the container with water. The rising water level would simultaneously touch the bottom of both objects. Now we let more water run in, until both objects are immersed by about 2 centimeters. What will happen?"

After this question has been discussed, we proceed to a new problem.

B. *Task III (Unsymmetrical Arrangement)*

The large wooden object is replaced by a small one which is of the same size and shape as the metal object, (but of different weight). Then the lever is balanced again. The lever arm with the light wooden body is considerably longer than the arm with the metal body (Fig. 5). — The subject is asked the same question, as above.

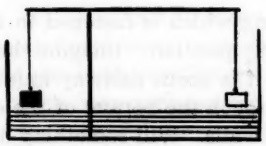

Fig. 5. Hydrostatic Lever Task III Unsymmetrical

SUBJECTS

All the thirty-eight subjects which participated in the In-

clined Plane Task, were also asked to solve Hydrostatic Lever Task II. Only twenty-one subjects took part in the experiments with Hydrostatic Lever Task III.

Implications of the solution

In what consists the essential difference between the Inclined Plane Task and Hydrostatic Lever Task II?

In physics we learn, that according to Archimedes' Principle all bodies, when immersed in water, appear to lose a certain part of their weight. However, it frequently does not become clear to us, that the "lost weight" corresponds with an equal increase in the pressure of the water. Most people never become aware that there is a problem in connection with the "lost weight". In our experiment, however, this problem has to be considered.

It is of course very well possible, that a number of subjects which solved the Inclined Plane Task (metal), would be able to solve this new task by means of their naive empirical knowledge. For them the metal hangs on the string. When immersed in water its pull on the string is decreased; the weight of the water in the container, however, is not increased.

With the wooden body things are, in the opinion of the subjects — quite different. This body "partly hangs on the string, partly floats" (this conception is frequently expressed) — and the floating part weighs on the water, the other part on the lever arm.

In all probability some subjects will immediately realize, that the weight of the container will increase and the corresponding lever arm will move downwards.

Hydrostatic Lever Task III (Symmetrical) can be solved through the naive empirical knowledge as well as through the knowledge of the Hydrostatic Principle. The situation can be interpreted in the following way: "Wood floats, metal sinks", therefore the lever arm with the wood will move upwards. The other interpretation would be: "The wooden body is larger, has a greater displacement which causes a greater loss in weight. The lever arm on this side will therefore move upwards".

The most difficult to anticipate is what will happen in the case of the unsymmetric arrangement. When the water level starts rising, both bodies seem to lose an equal amount of their weight, as they displace an equal amount of water. The balance

is nevertheless slightly disturbed, because the metal weight hangs on a shorter arm. The disturbance is, however, compensated through a slight change of position and the equilibrium remains undisturbed up to the moment when the wooden body begins to float, without submerging any deeper into the water, even though the water level keeps rising. Now the load on this side of the lever is zero and the metal body keeps — with the remainder of its weight — the longer lever arm in balance. If the water level rises still higher, the metal weight rises too and the arm with the wooden body gives downward. (If the metal body would not rise, but would gradually submerge deeper, then it would lose more and more of its weight and could not hold the longer arm in balance).

Results

The outcome of the experiments was as expected. A considerable number of subjects (Fig. 6) who understood and solved the Inclined Plane Task through their knowledge of the Hydrostatic Principle, were in the case of Hydrostatic Lever Task II not able to forethink that, through immersing of the metal weight, the water container becomes heavier. The knowledge of the Hydrostatic Principle was therefore not in both tasks functionally operative. On the other hand, it never happened, that the knowledge of the Hydrostatic Principle was operative in Hydrostatic Lever Task II, but not in the Inclined Plane Task. With some subjects this knowledge was functionally operative in both tasks (Fig. 6). Furthermore, of those subjects who could adequately apply their knowledge of the Hydrostatic Principle in Hydrostatic Lever Task II, only a few were able to make adequate use of their knowledge in Task III. It was, however, never observed, that a subject who was unable to solve Task II, reacted correctly to Task III. In short, not in every task can knowledge, acquired through meaningful learning, be applied with equal effect. In this respect subjects with and without adequate memorized knowledge reacted in the same way (Fig. 6).

In accordance with Bartlett's (1) and Wolters' (17) findings we may therefore speak of a greater or smaller functional generalization of meaningful knowledge. The functional generalisation is the largest with a subject who understands all three tasks, it is

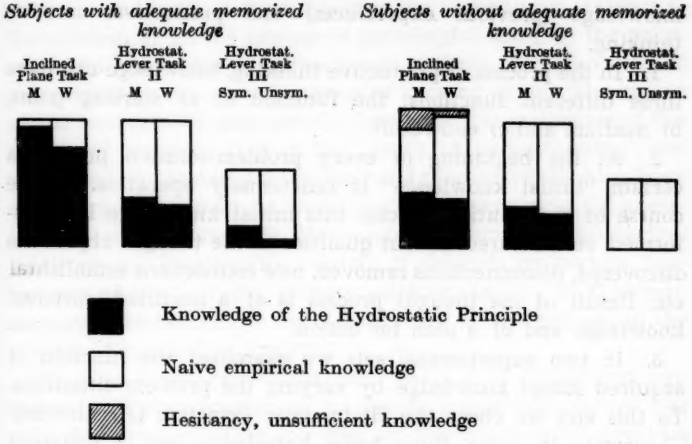

Fig. 6

smaller with one who solves the first two tasks and the smallest with one who solves only the Inclined Plane Task.

Discussion of the results

We previously raised the alternative question: Is the initial knowledge, from which in a certain problem situation the solution process takes its start, dependent on the prior assimilation of relevant learning material or on the present problem situation? Our experiments prove without any doubt, that it is dependent on the problem situation. Matters, which we have understood, do not constitute permanent thought material that is always at our disposal, in any problem situation in which it is needed. This, however, does not exclude the other alternative; this question cannot even be settled through our present experiments, because we only changed and checked the problem situations, but not the conditions of the learning process. Therefore the question remains open, whether or not the degree of the functional generalisation of knowledge is dependent upon certain peculiarities of the learning process — in other words dependent upon the way in which the learning material was psychologically assimilated.

Summary

The object of our inquiry is the functional relation between

knowledge (previous experiences) and productive, creative thinking.

1. In the process of productive thinking, knowledge can have three different functions: the function of a) starting point, b) medium and c) end-result.

2. At the beginning of every problem-solution process a certain "initial knowledge" is functionally operative. In the course of the solution process this initial knowledge is transformed, restructured. Latent qualities of the thought object are discovered, disconnections removed, new connections established, etc. Result of the thought process is a) a modified, enriched knowledge and b) a plan for action.

3. In two experimental sets we examined the function of acquired school knowledge by varying the problem situations. To this end we chose the Hydrostatic Principle (Archimedes' Theorem). In these three tasks knowledge had a different function. In the Inclined Plane Task it functioned as means to reach the goal. In the two other tasks (Hydrostatic Lever Task II and III) it was possible to forethink, on the basis of the initial knowledge, what would happen.

4. Two different kinds of knowledge functioned as initial knowledge: a) the naive empirical knowledge: "Wood floats, metal sinks" and b) the knowledge of the Hydrostatic Principle: "In water all physical bodies appear to lose a certain part of their weight".

5. Not with all subjects, who could recall it and reproduce it verbally, was the knowledge of the Hydrostatic Principle functionally operative.

6. Knowledge of the Hydrostatic Principle was operative with many subjects who had "forgotten" their school knowledge and were not able to reproduce it verbally.

7. The degree in which the knowledge of the Hydrostatic Principle is functionally operative, differs within the series of the three problem situations. With some of the subjects this knowledge was operative only in the Inclined Plane Task, with others in the Inclined Plane Task as well as in Hydrostatic Lever Task II and in very few of the subjects in all three tasks.

8. Where the functional operation of this knowledge ends, there usually the naive empirical knowledge assumes the function of the initial knowledge.

9. On the basis of these findings the following hypothesis is formulated: a) In the process of meaningful learning the naive empirical knowledge is restructured. b) Such restructuration of reorganization can comprise areas of varying extent. The extent of the restructured area and the functional generalization of the results of learning, that is the "depth of understanding", are correlated. c) Meaningful learning and productive thinking have something in common. In both instances a piece of initial knowledge is restructured. d) Meaningful learning and productive thinking are functionally related; it depends on certain particulars of the learning process whether, and to what extent, acquired knowledge can be utilized, that is: reorganized in the process of productive thinking.

10. The hypothesis formulated under 9 is not as yet empirically substantiated in all the details. Empirically proven was only, that the productive processes are dependent on the structure of the actual problem situation.

11. The usefulness of the method of introspection for the solution of thought problems is a very limited one. Knowledge, though functionally operative, generally does not reveal itself phenomenologically. Its transformation, reorganization in the process of learning, of comprehension and of problem solving cannot be disclosed through introspection. Only the objective results gained through systematic, planful variation of the experimental conditions make it possible to reconstruct knowledge and its fate.

12. Memorized, verbally reproducable knowledge — to which the school attaches so much importance — is often not functionally operative; knowledge which is not accessible to the representative memory, can be functionally operative in solving a thought problem or in comprehending something new. Essential for the effectiveness of knowledge is not how easily it can be reproduced, but how well it has been assimilated, that is, understood originally.

13. Terminology and classification of types of knowledge:

a. In regard to its manifestation:

1. Knowledge that reveals itself phenomenologically, i.e. is accessible to the representative memory.
2. Knowledge that is functionally operative.

b. In regard to the way in which it was acquired:

1. Naive empirical knowledge.
2. Knowledge acquired through learning.

c. In regard to its function in the process of thinking:

1. Initial knowledge.
2. Medium of thinking.
3. Result of thinking.

ZUSAMMENFASSUNG.

Gegenstand der Untersuchung ist der funktionelle Zusammenhang zwischen Wissen, bzw. früheren Erfahrungen und dem produktiven schöpferischen Denken.

1. Im Prozess des schöpferischen Denkens kann das Wissen drei verschiedene Funktionen haben: Das Wissen kan die Funktion a) des Ausgangspunktes; b) des Mittels und c) des Resultats haben.

2. Am Anfung jedes Problemlösungsgeschehens ist ein gewisses *Ausgangswissen* funktionell wirksam. Im Prozess des Lösungsgeschehens wird das Ausgangswissen verändert, umstrukturiert. Latente Qualitäten der Denkgegenstände werden entdeckt, oder Trennungen werden aufgehoben, neue Zusammenhänge werden hergestellt, usw. Resultat des Denkens is a) ein verändertes (bereichertes) Wissen und b) Plan zu einer Handlung.

3. In zwei Hauptversuchsreihen wurde die Funktion des erworbenen Schulwissens in verschiedenen Problemsituationen untersucht. Gewählt wurde das Wissen um das hydrostatische Grundgesetz (Satz van Archimedes). Das Wissen hatte in den drei Problemsituationen verschiedene Funktionen. Bei der einen Aufgabe (Schiefe-Ebene) hatte das Ausgangswissen die Funktion, dass mit seiner Hilfe das Mittel zur Zielerreichung gefunden werden konnte. Bei zwei anderen Aufgaben (Hydrostatische-Hebel II und III) konnte auf Grund des Ausgangswissen vorausgedacht werden, was eintreffen wird.

4. Die Funktion des Ausgangswissens haben zwei verschiedene Wissensarten erfüllt. A) das naive Erfahrungswissen: "Holz schwimmt, Metall sinkt" und B) das hydrostatische Wissen: "Im Wasser verlieren alle Körper scheinbar einen gewissen Anteil ihres Gewichtes".

5. Das hydrostatische Wissen war nicht bei allen Vpn funktionell wirksam, die sich an das physikalische Gesetz erinnern und es verbal wiedergeben (reproduzieren) vermochten.

6. Das Wissen um das hydrostatische Gesetz war auch bei einigen solchen Vpn funktionell wirksam, die das Schulwissen "vergessen" haben, es verbal nicht wieder geben (reproduzieren) vermochten.

7. Die funktionelle Wirksamkeit des Wissens um das hydrostatische Gesetz nimmt in der Reihe der drei Problemsituationen stetig ab. Bei einem Teil der Vpn war dieses Wissen allein bei der Schiefen-Ebene, bei einem

Teil bei der Schiefen-Ebene und der Hydrostatischen-Hebel II-Aufgabe und bei ganz wenigen bei allen drei Aufgaben funktionell wirksam.

8. Wo die funktionelle Wirksamkeit des Wissens um das hydrostatische Gesetz aufhört, dort tritt das naive Erfahrungswissens als wirksames Ausgangswissen in Kraft.

9. Auf Grund dieser Befunde wird die Hypothese aufgestellt: Im Prozess des verständigen Lernens wird das naive Erfahrungswissen umstrukturiert. Das verständige Lernen ist analog dem produktiven Denken. Ein Stück Ausgangswissen (jenes Wissen, das man vor dem Lernen gehabt hat) wird umorganisiert. Die Umorganisierung kann verschieden grosse Bereiche ergreifen. Der Grösse des umorganisierten Bereichs ist die funktionelle Generalisiertheit der Lernwirkung, bzw. die "Tiefe des Verständnisses" zugeordnet.

10. Es wird zugegeben, dass die Hypothese empirisch noch nicht genügend gesichert ist. Es ist empirisch sichergestellt, dass die produktiven Prozesse von der Struktur der aktuellen Problemsituation abhängig sind. Ob die produktiven Prozesse, wie Umorganisierung, von den Besonderheiten des Lernprozesses (bzw. der Vorgänge in der Erfahrungssituation) auch abhängig sind, ist eine unentschiedene Frage.

11. Die Brauchbarkeit der Selbstbeobachtungsmethode bei der Lösung denkpsychologischer Probleme ist sehr gering. Das funktionell wirksame Wissen ist meistens phänomenal nicht gegeben. Erst das systematische Variieren der Versuchsbedingungen ermöglicht es, das Wissen und seine Veränderungen bei der Vp zu "rekonstruieren".

12. Das gedächtnismässige Wissen, worauf die Schule ein so grosses Gewicht legt, ist oft funktionell unwirksam. Für das repräsentative Gedächtnis unzugängliche Wissen kann bei der Lösung eines echten Denkproblems oder beim Verständnis einer Situation funktionell wirksam sein.

13. Zur Terminologie und Einteilung der Arten des "Wissens": (a) mit Rücksicht auf das Auftreten: 1. Phänomenal gegebenes, bzw. für das repräsentative Gedächtnis zugängliches und 2. funktionell wirksames Wissen. (b) Mit Rücksicht auf die Weise, wie das Wissen erworben wurde: 1. naives Erfahrungs- und 2. erlerntes -Wissen. (c) Mit Rücksicht auf die Funktion im Prozess des Denkens: 1. Ausgangswissen; 2. Mittel des Denkens (Vehikel); 3. Ergebnis des Denkens.

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An experimental contribution to the psychology of the visual perception of velocity

CONFIGURATIONS IN THE PERCEPTION OF VELOCITY

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I

The present investigation represents only a part of a more comprehensive attempt to gain some conception of the laws which determine the genesis of configurations, inter alia, in the perception of velocity.

In a number of experiments which were conducted with this aim in view, it was repeatedly observed that an object which actually had the same physical velocity, appeared to be moving at different velocities under various experimental conditions. Briefly stated, the phenomenon observed, was that the velocity of the object seemed to increase considerably when another moving object was introduced into the same homogeneous and restricted visual perceptual field.

The very scanty literature that exists on the perception of velocity, does not contain any account of investigations dealing with phenomena of this character. The investigation, which will be described below, was carried out because the author's own approach to the problem, as mentioned above, was such that he felt it almost indispensable for him to ascertain the unknown laws that govern this field; and besides, he was of the opinion, that elucidation of this matter must prove of considerable interest in general.

The question can be stated thus: Is the phenomenal velocity of an object in motion that is visually perceived, affected by the motion of another object in the same perceptual field? If this is the case, what are the factors that determine this influence?

II. Previous Investigations that bear on our Problem

It was observed, already at a relatively early date, that the same physical velocity can be experienced as having different values. Thus, about 1880, E. Fleischl [1]) and H. Aubert [2]) found, independently of one another, that the velocity of a rotating kymograph was estimated differently when the attitude of observation was changed. If the movement itself is watched, the velocity appears to be considerably lower than when the eyes are fixed and a point at rest which is near to the moving object. The phenomenon in question, which cannot be said to have received a definitive explanation, has been discussed later on several different occasions. Attempts at interpreting this phenomenon have been made by W. Stern [3]), W. Wundt [4]), R. Dodge [5]) and W. Filhene [6]) among other investigators. In scientific literature it is usually referred to as the Aubert—Fleischl paradox. It will play an important role in our discussion.

Let us now consider more recent experiments. In order to establish a basis for psychotechnical tests, A. Dembitz [7]), a civil engineer, examined men's capacity to appraise velocity. Some of his results are of interest to us, in as much as they lead to conclusions that, in principle, coincide with our own experimental results. His experiments largely consisted either in allowing O to observe a velocity and then trying to reproduce it; or in letting him note the time an object, which during part of its course has been concealed by a screen, is assumed to pass a certain point on the screen (i.e. a time estimate).

By far the most important contribution to the investigation of

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[1]) E. Fleischl. "Physiologisch-optische Notizen". *Sitzungsbericht Akad. Wiss. Wien, Abt. III.* 86, pp. 17—25 (1882).

[1]) H. Aubert. "Die Bewegungsempfindung". *Pflügers Arch.* 39, pp. 347—370 (1886); 40, pp. 459—480 (1887).

[3]) W. Stern. "Die Wahrnehmung der Bewegung vermittelst des Auges". *Zeitschr. f. Psychol.* 7, pp. 321—385 (1894).

[4]) W. Wundt. *Physiologische Psychologie.* 5 Aufl. 1902, Bd 2, pp. 579 f.

[5]) R. Dodge. "The participation of eye-movements in the perception of motion". *Psychologic. Rev.* 11, pp. 1—14 (1904).

[6]) W. Filhene. "Ueber das optische Wahrnehmen von Bewegungen". *Zeitschr. Sinnesphysiol.* 53, pp. 134—145 (1922).

[7]) A. Dembitz. *Beiträge zur exp. Unters. der Bewegungswahrnehmung durch das Auge 1927.* (diss.)

the perception of velocity, has certainly been made by J. F. Brown (1928—1931) [8]). Brown found that the same physical velocity can have quite different phenomenal values, which depend on the conditions under which perception takes place. He pointed out a number of contributory factors. Firstly, there is the influence exerted by the dimensions of the field and of the moving object on the velocity experienced; these conditions were studied by him; but he also examined the part played by other factors such as illumination and inhomogeneity in the field of motion. He succeeded in demonstrating, for example, that if all the dimensions both of the surrounding field and of the object are reduced to half their original size, then the velocity appears to be nearly doubled. Brown's experimental method consists in the comparison of two light fields situated on opposite sides of an otherwise dark room, and with O sitting in between the two fields and at an equal distance from either of them. He has only investigated the velocity of a single object moving through a homogeneous field. Brown has also furnished a summary of the literature concerned with the perception of velocity up to 1931.

III. Method of Investigation

In order to be in a position to make quantitative determinations concerning the phenomena to be studied, the investigation was based on a comparison of two fields which were exactly alike, except for the two factors that were to function as variables, that is to say, as regards the number of objects and their velocities. In the great majority of the experiments, one of the fields contained a single object (a small black circular plane) in motion, while the other field contained two such objects.

In accordance with the usual methods employed in psychophysics, one field had a constant velocity, and it was O's task to find in the other field a velocity that appeared to be equal to the constant one. When choosing between different psychophysical methods for determining phenomenal identity,

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[8]) J. F. Brown. "Ueber gesehene Geschwindigkeiten". *Psychol. Forsch.* 10, pp. 84—101 (1928).

"The Visual Perception of Velocity". *Psychol. Forsch.* 14, pp. 199—232 (1931).

the adjustment method was found to be the most suitable. By manipulating a wheel, O can constantly alter the velocity in the variable field to cover a very wide range.

As is usually the case, a high and low initial velocity were used alternatingly; and it was Os aim merely to try and attain the same velocity in both fields.

In two preliminary experiments, however, a somewhat different method was employed. Thus, in accordance with the method of limits, an upper as well as a lower difference threshold, with both descending and ascending series, was noted. These values were obtained by means of a bound adjustment procedure where O by turning the above-mentioned wheel only in one direction seeks to find the limits. It was possible here to determine directly the width of the threshold. In the principal method, which is relatively simple in comparison, the deviation denotes a threshold value.

No special principles were adopted for the selection of observers. For the most part, these were personal acquaintances, the majority of whom have nothing to do with experimental psychology. Some of the members of the much respected Uppsala messengers' corps also made praiseworthy contributions.

When the experimental series were planned, we aimed at having any possible practice factor well under control. This was achieved in two ways. Firstly, three observers took part in practically all the experiments, while the others were changed each time. We tried to have at least two well trained and two inexperienced Os for each experiment. In this manner Os were being constantly changed; and 28 persons actually took part in the experiments. The correlative time sequence of the various repetitions of the same experiment were also given for each O; while for those who took part in several experiments, their order was noted.

During the course of the investigation, however, it became evident that practice played a very insignificant role in the primary result. The effects of greater experience were mainly visible in a distinctly more rapid performance of the task, and possibly also in a somewhat more restricted distribution of the partial results. Personal qualifications such as thoroughness-carelessness, certainty-uncertainty etc., were more important than the element of practice.

IV. The Arrangement of the Experiments

The apparatus that was employed for these tests was not specially constructed for this purpose; but was a kind of all-round apparatus which was used for the more comprehensive investigation of configurations during motion, which was alluded to in the introduction. With it, it is possible to procure relatively intricate and various types of motion for a whole series of objects of arbitrary shape and size. A detailed description of this apparatus in its entirety will be available when the investigation in question is published. At present we will only give a schematic account of those functions of the apparatus which were employed in connection with the experiments now under consideration.

The apparatus is a type of projector. It projects into a translucent screen a picture of the objects which have been produced on clear celluloid discs. The objects are made of small dark circular planes. The celluloid discs can be made to move horizontally backwards and forwards, whereby the circles projected on the screen make the same movements. The amplitude of the motion can be accurately determined, and is very exact.

Fig. 1 is a diagram of the contrivance employed. (a) is the lantern box with condenser; (b) the screen to which the lens is attached, together with a shutter immediately in front of it; (c) is the projection screen which the observer sees from the side that is visible in the figure. One of the celluloid discs is indicated as a rectangle by means of dotted lines at (e). The celluloid disc is attached by a clamp device to an arm (f) which can be made to oscillate from the axle (*g*). The point (*g*) lies on a vertical line which cuts the central axis of the projection system at (h). The arm (f) is connected by means of a twin-armed lever (j) which can move about the axle (1), to an eccentric disc (i). When the eccentric disc is made to rotate the arm-celluloid disc system is thereby given an even oscillatory movement from the axle (g), and a point (h) on the celluloid disc is the end point of an imaginary pendulum that is vertically suspended. Since the length of this pendulum is very great in comparison with the maximum oscillation (100 : 3), the movement at (h) is practically rectilinear. The point of attachment (k) on te lever (j) is free to move along its lever arm. By this means, the lever arm can be

shortened or lengthened - and the amplitude of the pendulum is thereby simultaneously increased or respectively decreased.

The projection screen (c) was required to effectively prevent the possibility of simultaneously comparing the two fields where

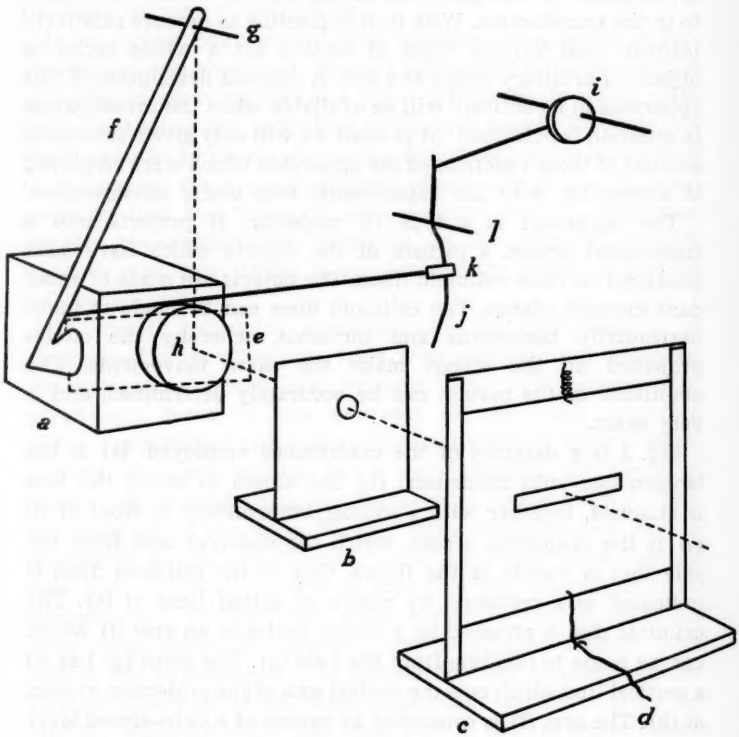

Fig. 1.

the motion was taking place. At the same time it had to enable O not only to alternate quickly and easily from one field to the other, but also to make it possible for him to change freely at any time desired. The contrivance described below, was found to fulfil the required conditions very satisfactorily:

A screen 350 × 180 mm. its vertical sides are furnished with a groove along which a white cardboard disc (260 × 180) can

pass. This disc is fitted with a rectangular "window" (150 × 24 mm.) which has been cut out. This is shown in Fig. 1 at (c). The disc is kept in a fixed upper position by means of a little spring, but it can easily be moved to a lower position by O pulling the cord (d). The distance between the upper and the lower position is 28 mm.

The movable objects are projected on to the back of the screen in such a way that the object or objects which belong to the one field become visible along the central horizontal line of the window when the disc is in the top position, and an object that belongs to the other field is visible along the same line when the disc is moved to the bottom position. Thus by drawing or releasing the cord, O can at pleasure, compare the two fields of motion in succession without practically any time interval; but he can never see them simultaneously. The contrivance also possesses a distinct advantage from an experimental technical point of view, in as much as the motions which are to be compared, can be observed from the same window on the same screen. Consequently, all possible disturbances due to shifting, when it is a question of background, etc., are definitely eliminated. The only external difference is the 28 mm. difference in height.

The lighting in the room where the experiments were conducted was subdued. O was seated 75—100 cm. from the screen. The regulator for the velocity of the apparatus was connected to a wheel which was fixed to the experimenter's seat. The latter has himself to adjust the velocity in the variable field so as to obtain a phenomenal *equality* with the velocity in the constant field. By this means, not only has the greatest possible precision been obtained, but every kind of suggestive influence emanating from the experimenter has been eliminated. The part played by the latter has been restricted to setting the initial velocity, and reading a graduated scale or a stop-watch device, once O is satisfied with his results. It has been made impossible for O who has to repeat the same experiments to endeavour quite mechanically to find the same positions on the wheel. This has been accomplished partly by the gears which require 5—20 turns of the wheel for a new adjustment; and also by the construction of the apparatus which enables the experimenter

to choose both the initial velocity and the change in gear for every new trial.

A technical description of the device by which this manipulation is effected is omitted, since it would prove relatively complicated and could hardly be expected to assist the reader.

In the experiments, if nothing is said to the contrary, the objects whose velocity has to be determined were black circular planes, 4 mm. in diameter. These will subsequently be termed *objects, elements,* or simply *points.*

The motion track in the constant field was 20 mm., and the velocity most frequently used was 20 mm./sec.

The field of constant velocity will subsequently be termed field I, and the variable field, field II. When phenomenal equality between the two fields has been attained, the physical velocity is measured, and a quotient II/I representing the values obtained is then called the experimental result [9]). If the physical velocity is the same, this quotient will be equal to 1,00 when expressed as a decimal fraction.

Now as regards the character of the motion itself. It has already been pointed out that we have to deal with a translatory oscillating motion, and that the path of motion has a horizontal extension. But it should be noted that the motion has *not* a constant velocity along the whole length of the path with sudden turns. By choosing an eccentric disc for the conversion of the rotary movement into a translatory one, we obtain in its place, what is known as a simple harmonic motion. In mechanics, as is well known, this motion is rectilinear, oscillatory and periodic. Its velocity is constantly accelerating either in a positive or a negative sense, and is at its height at the middle point of the path. It is zero at the turning points, and constant transitions take place in between the latter. This type of motion can be graphically represented by a sine curve.

When an O is shown this kind of motion passing through a homogeneous field, and is asked how the velocity of the object behaves if different parts of the path of motion are compared

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[9]) For the sake of clarity, the terms physical velocity and phenomenal velocity or experienced velocity will be used consistently. Physical velocity, naturally, refers to the velocity measured in mm./sec., and the other terms are used without distinction in reference to the subjective impression of velocity.

with one another, or in other words, if the velocity changes along the path, practically without exception the same answer is received: the point moves more slowly just at the turning point; but otherwise its velocity is constant.

This characteristic, which is equally shared even by those who are fully aware of the great change in velocity that actually takes place, is in itself a very surprising and noteworthy phenomenon. It will be gone into more fully at a later stage. This phenomenon is mentioned here in order to indicate the perceptual character of the type of motion employed.

From a physical point of view, velocity is a function of space and time. If it is desired to vary the velocity in connection with the experimental apparatus that has just been described, this can be done by employing either of two different methods: The time marking, in accordance with the frequency (the number of turns per second at one turning point), may be kept constant, while altering the length of the path of motion, the amplitude; or the amplitude can be kept constant, and the velocity changed by altering the frequency. The first alternative is effected technically by moving the clamping device (k) along the arm (j) which leads to a change in the amplitude. According to the second method, the velocity of the revolving eccentric disc is altered which in turn brings about an alteration in the frequency.

It must be clearly pointed out, however, that in both these methods, the variation in velocity is accompanied by an alteration in another perceptual content, i.e. the distance between points in space or time. Though this is necessarily connected with the velocity, it is nevertheless, *in itself,* quite different from the actual velocity. Consequently, proper care must be taken to definitely determine that phenomenal equality as regards the velocity has really been achieved, and not to mistake this for equality as regards the time interval or, respectively, the length of the path.

The apparatus is driven by electric gramophone motors with centrifugal regulators. When the velocity was varied by changes in amplitude, a single motor was used; while two motors were employed for the method entailing alterations in frequency. Since in the latter case, the reliability of the result is directly dependent on the speed constancy of the motors at different velocities, these were carefully adjusted and thoroughly tested.

The maximum variations in velocity that have been measured in this connection were considerably below ± 0.5 %. This shows that precision had been definitely attained. The constancy both for one revolution, and also for a longer period of time was quite easily and clearly controlled before each experiment. A stroboscopic disc was attached to each of the motor axels, and this was sensitive to any possible fluctuations in velocity when illuminated by a 50-periodic light.

V. The Length of the Track as a Variable of the Velocity

We propose to commence by considering a series of experiments where the velocity is affected by changing the amplitude of the track. This signifies, as was mentioned above, that the time intervals between the turning points are always constant and exactly alike in both the constant and the variable field. When O alters the velocity in the variable field, this is effected by either increasing or decreasing the track (a fact, which strangely enough, the majority of Os do not pay any attention to). Technically, as has already been mentioned, this change in velocity is effected by moving the point of attachment (k) along the lever arm (j) which regulates the amplitude (see Fig. 1).

Our fundamental question is concerned with a comparison between the experience of velocity, when the field contains a single object in motion, and when it contains two such objects with mutually different states of motion. In order, however, to test the accuracy of our methods of measurement, we intend to start with an experiment where each of the fields contains a single object. By letting O try to attain the same velocity between the two fields, which are otherwise exactly alike, we obtain a gauge of the sensitiveness for differences in velocity under the prevailing experimental conditions.

Experiment 1. In each field an object is exposed that consists of a black point 4 mm. in diameter and moving in accordance with the description on page 32 (horizontal path, oscillating motion). In the constant field (field I) the length of the track was 20 mm., while in the variable field it was approximately twice as long at the initial stage. Each cycle took 2 seconds, and the velocity in the constant field was thus 20 mm./sec. The

middle point of every track coincided with the middle point of the "window". Fig. 2 shows the two fields. The track is indicated by a dotted line, and the object is represented by a black circular plane at the middle point of the track.

Fig. 2.

The experiment was carried out in accordance with the method of limits as stated above. Five Os took part, and in all 64 values were noted. The results are given in table 1.

In this table, as well as in the following context, the mean (M) denotes the arithmetical mean of the velocity quotients II/I obtained during the experiments. When calculating the deviation (S.D.) and the standard error, (S.E.) the size of the material has been taken into account by employing the formula $\sigma = \sqrt{\frac{\sum (X-M)^2}{n-1}}$ to compute the deviation in experiments with less than 25 repetitions.

TABLE 1

Series	Number of Os	Number of trials	M.II/I	S.D.	S.E.
↑ equal	5	16	0.965	0.029	
↑ upper limit	5	16	1.10	0.052	
↓ equal	5	16	1.02	0.074	
↓ lower limit	5	16	0.90	0.064	
↑↑ + ↓↓	5	64	0.996	0.089	0.011

Of the five Os who took part in this series of experiments, two had had previous experience with similar tests; as for the rest, not only was this something entirely new, but they lacked any practical experience in connection with experimental psychology. The results of only one of the latter showed relatively wide fluctuations (0,73—1,25); while those of the others,

when we bear in mind their individual dispersion, were remarkably similar. As shown in the table, this experiment indicates that surprisingly small differences in the character of the motion, which we have here varied, are registered by Os.

The author originally considered that there was an advantage, at least in some cases, in determining the width of the difference threshold by the method of limits, and consequently this method was employed in the preliminary test. During the course of the investigation, the adjustment method, which has been described above, proved the most satisfactory for the purpose in hand; and hence, this method would doubtlessly also have proved suitable for the preliminary experiment. The width of the threshold would then also there have been ascertained by means of the deviation. It did not appear expedient, however, to repeat the test merely for the sake of uniformity. Instead of this, the last row in Table 1 gives the common mean for all the different values and their deviation in respect of this mean. The only essential difference between this result and the one the adjustment method would have given is, that here the deviation must be assumed to be greater, since half the values obtained show an incipient difference in place of identity.

Before passing on to the specific experiments let us first examine a question which is of decisive significance for the whole investigation: whether the *length of the track* is the actual determinant of equality and not the velocity of the motion.

We can point to a fact that is intimately connected with this: under the given experimental conditions, when equality in velocity is attained, equality regarding the length of the track is achieved simultaneously.

Is it this latter equality that determines the experience of equality? Or is it, at any rate, an essential factor? The instructions given to Os certainly advise them to try only to compare the velocity, and not to concern themselves with other equality factors that may be present. As a matter of fact, all O's were convinced that it was only the velocity that they had estimated. A subjective conviction of this nature should not, however, be regarded as constituting a proof. The need for control in this respect remains unaffected.

Experiment 2. In order to establish this kind of control, a series

of experiments were conducted which represent an exact repetition of Experiment 1, with the one exception that a shutter was introduced immediately in front of the lens of the apparatus. This shutter was driven by the axle to which was attached the eccentric disc belonging to the apparatus; and it was therefore synchronized with the iteration frequency of the motion. It was so adjusted as to "cut off" the motion in one direction, and both the turning points. This is shown in Fig. 3. The arrows indicate

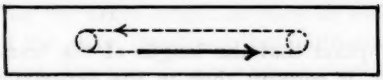

Fig. 3.

the direction of the motion; continuous line represent the exposed portions of the track; while the dotted line show the parts that were concealed by the shutter.

The field consequently is illuminated and darkened at regular intervals. During the illuminated phase the object is always seen moving towards the same goal [10]). In the case of the original, oscillating motion, the turning points, where an instant's rest took place, became the fixed points of the track; and it is these that make it possible to obtain a relatively good appraisal of the length of the track. When these fixed points were concealed by the shutter, the result was an inevitable and fundamental diminution in the sense of certainty with which the length of the track could be estimated.

The fact that no starting point can be observed, and that at the very outset of the illuminated phase, the object appears to pass with a constant velocity, plays an important part in this respect.

If the equality of the length of the track was a contributory factor in Experiment 1, then a lower degree of precision must be expected for Experiment 2, when compared with Experiment 1.

In Table 2, the experimental results of the shutter variation have been combined, so as to accord with the summaries given

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[10]) The direction from left to right was chosen because it corresponds with the occidental way of reading from left to right. An experiment with the direction reversed gave, however, a quite consistent result. It would also be of interest to compare horizontal and vertical motion respectively.

in the last row of Table 1. As shown in the table, the variant in question has given a somewhat lower mean [11]), and the dispersion is less noticeable.

TABLE 2

Number of Os	Number of trials	M. II/I	S.D.	S.E.
4	32	0.975	0.076	0.013

If the assumption that the length of the track is an actual equality factor, is correct; then in the experiment now under consideration, it would be natural to expect an increase in dispersion, as a result of the above-mentioned diminution in precision, when it is a question of estimating the length of the track.

The dispersion that we have obtained, however, shows without any doubt, that the precision of the active factor has not been diminished. As a result of our experiment with the shutter, which accords with the observers' assumption, we can state that it is, in all probability, the experience of velocity that is the determining factor in the comparison of the two fields. For the only variable factors in the experiment under consideration, were the velocity of the object and the length of the track.

As we shall see later, this question of the part played by the track as an index of velocity will, however, reappear in connection with another experiment. We shall then have an opportunity of testing this from a different angle.

If we consider the joint results of these two experiments they entitle us to draw three conclusions which may serve as a starting point for the following investigations:

1. With the help of our experimental device, we can detect, with a high degree of precision, the equality and differences in the varying qualities of the motion that takes place here.
2. The observed experience of equality or difference refers to the phenomenal velocity.
3. The objective identity between the velocities in the two

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[11]) The difference is not statistically significant ($2 \pm 1{,}67$).

fields of motion which are exactly alike, is experienced as equality.

With the insight that we have now gained as a background, we can turn to the experiments which have a direct bearing on the question formulated in the introduction. We shall commence with the following basic experiment.

Experiment 3. An additional object is introduced into the constant field. In this case it is also a black circular plane 4 mm. in diameter. It has the same length of track as the original object (20 mm.), and the same frequency, but with reversed direction of the movement. The two objects were so placed in the common field, that for each of them, one of the turning points was situated in the middle of the field. Furthermore, these two turning points that coincided with each other, were made to cover one another exactly. Fig. 4 is a diagram of the movement combination. Thus, from their respective turning points on the periphery, the two points move simultaneously towards the centre, meet, overlap, and immediately return, etc.

The variable field (field II) still only contained a single object. The centre of its motion coincided, as in the former experiment, with the middle point of the field. It has, at the commencement of the experiment, the same length of track as each of the objects in field I, and also the same velocity as they have. And naturally, it also has the same iteration frequency.

As in the previous experiment, O is free to observe the motions without having to adopt any definitely prescribed observation attitude.

The adjustment method, which was described on page 27 f, was used in this and all the following experiments.

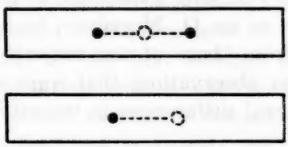

Fig. 4.

For those who are not initiated, the results of this experiment must seem very surprising. All Os were convinced that the single point (field II) had a much lower velocity than the two

points in field I. Only when the original velocity of the single object has been nearly doubled, does there appear to be phenomenal equality between the two fields. Hence, this experiment gives a distinctly positive answer to the first question in our formulation of the problem (page 25). The figures relating to the experiment, are given in Table 3. As will be remarked, the experiment was carried out by ten Os, and there were in all 44 trials.

TABLE 3

Number of Os	Number of trials	M. II/I	S.D.	S.E.
10	44	1.94	0.16	0.024

The stable result with its astonishingly slight deviation from the mean, as shown in the table, permits only of one conclusion: that here we are dealing with a generally valid and very exact perception factor.

The results have been confirmed beyond question. The experiment was made with an apparatus that functioned very accurately, and it was carried out under the strictest control. The number of Os who took part, and the number of trials made, were in this case, more than sufficient to insure a reliable result. Besides, Os have been tested repeatedly at relatively long intervals (2—33 days); and the table is based on this material, which as regards time is widely scattered. Some of the Os had not previously had anything to do with experimental psychology; while others were already intimately acquainted with the subject. None of the Os had any knowledge of the purpose of the investigation; or of the real velocities; this at least applies to the initial experiment. The only exception to this, was the author who also took part as an O. Moreover, his series of results did not differ at all from those of the majority of the other Os. This fact, and other observations that were made, indicate that knowledge of the real differences in velocity did not affect the results.)

The question that now presents itself is: what is this remarkable effect due to? In order to obtain an adequate answer, three possible hypotheses will be discussed.

1. Under conditions of unrestricted observation the object

observed is usually persued by the eyes. That is the natural observation attitude for field II. In field I, on the other hand, it may possibly be assumed that an O, on account of the combination of movements, might spontaneously fix his gaze on the meeting point itself which is the symetrical point of the double motion. According to the Aubert-Fleischl paradox, the ratio would then be approximately 2 : 1, when the different objects have the same physical velocity. (According to J. F. Brown's calculations it is 1.4 : 1).

2. The question of the length of the track as an unconscious equality factor, again comes into prominence here, and the refutation of this that resulted from the preliminary test does not entirely cover the present combination of movements. For the two tracks in field I appear to be a single common track of double the length. The peripheral turning points of the two objects are seen at the same instant; and it is this that gives rise tc the unequivocal impression referred to. Only when the distance between the turning points of the single object in field II is the same as this common distance in field I, will the impression of equality be observed. But this equality signifies, under the given conditions, a velocity ratio of 2 : 1.

3. Finally, one may be led to assume that the velocity of the two elements in field I is no longer experienced as independent units, even if an observer without reflecting, takes this for granted. Instead, they appear to form a "velocity gestalt" with doubled intensity in relation to its elements. Changes in the distance between the elements would in that case be the cause of the impression of velocity, and not the elements' relationship to the surroundings that are at rest.

Let us now consider systematically these various possibilities that may be put forward as explanations. At first, there seems to be much in favour of the first attempt at finding an explanation. This alternative, would enable us to explain an otherwise unheard of and extremely remarkable phenomenon, by referring it to a fact that is already well established. The following experiment will elucidate this point:

Experiment 4. In order to ascertain whether the phenomenon could be explained by different attitudes of observation in relation to the two fields, Experiment 3 was repeated, but with

the following alterations: The object's centre of motion in field II was placed directly under the corresponding centre of the object to the left in field I. A spot light with the same degree of luminosity as the field itself, was placed a centimeter above the upper edge of the "window", and along its vertical middle line. O was requested to fix his gaze constantly on this spot, and only to estimate the velocity when he could clearly see the spot light.

When the different objects in both the fields have the same objective velocity and amplitude, and if the retina is assumed to be completely at rest, then the conditions pertaining to the velocity perception on the basis of the Aubert-Fleischl paradox, must be exactly the same in both fields. The track of the lefthand object in field I must coincide on the retina with the same points as the track in field II. Hence, also in this case, the various distances from the fovea do not exert any influence.

Table 4 gives the results of all the 40 trials, where, in accordance with usual practice, high and low initial velocities alternate. Three well-trained Os took part in the experiment. It should be pointed out that all the Os found that the fixation requested, was an extremely artificial attitude, and one very difficult to carry out completely. Consequently, in all probability, the result does not fully correspond with the given presuppositions.

TABLE 4

Number of Os	Number of trials	M. II/I	S.D.	S.E.
3	40	1.67	0.14	0.022

A comparison between the means in tables 3 and 4, shows that there is an important difference with regard to the experienced velocity relations between the two fields, with unrestricted observation on the one hand, and the same thing, with fixed gaze, on the other. (The difference between the total means of the two tables amounts to approximately 8 times the standard error of the difference: 0,033).

Nevertheless, this difference cannot even approximately furnish the explanation we are looking for. It only consists of a full quarter of the original difference in velocity between the

two fields. If the fixation had been the decisive factor, then the quotient values in Table 4 would not have been reduced by merely a quarter of the difference between the quotients of Experiment 1 and Experiment 3; but, of course, by almost the whole of this difference. In other words, the quotient would not have deviated to a greater extent from the value 1,00, than could have been explained by the Os' inability to keep their eyes gazefully fixed on the given spot, when observing field II.

The obvious differences that have been observed, make it clear, on the other hand, that we may here still be dealing with a contributory cause [12]) of the phenomenon under consideration.

In this way, the results of Experiment 4 furnish us with a memento for the following investigations. The Os continued to be restricted by a given observation attitude, and were not at liberty to choose this for themselves. Otherwise one may be led into a serious source of error [13]).

We shall now proceed to review the second form of explanation that has been suggested. Can the remainder of the impression of equality, when the objective velocity ratio is 1 : 2, depend on the phenomenal equality of the amplitude in the two fields? Partly in order to find an answer to this question, but also so as to vary the original experiment from a more general point of view, three experiments were conducted where the constellation in field I differed somewhat from that of the basic experiment.

Experiment 5. The experimental conditions correspond entirely with those of Experiment 3, except for the fact that the two objects in field I are separated to such an extent, that the distance between the turning points situated in the centre of the field (which formerly coincided with one another) is now 15 mm.

Four Os took part in the experiment, on of them for the first time. Altogether 28 values were noted. As shown in Table 5, the effect of the alteration was that the quotient sank to 1,56.

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[12]) By way of a supplement, it would prove of great interest, by photographic means to ascertain the movements of the eyes during the free observation of the two fields. Unfortunately, the necessary technical equipment was not available.

[13]) The usual instructions encourage the Os to let their eyes follow the movement in a natural way, and neither to concentrate their gaze on a fixed point, nor to attempt long-distance accommodation.

TABLE 5

Number of Os	Number of trials	M. II/I	S.D.	S.E.
4	28	1.56	0.19	0.036

Experiment 6. This resembled Experiment 5 in every way, except for the minimum distance between the two objects being 30 mm.

TABLE 6

Number of Os	Number of trials	M. II/I	S.D.	S.E.
3	12	1.32	0.18	0.052

Table 6 represents the results obtained in this experiment. The number of trials is perhaps slightly too low to permit of more differentiated conclusions being drawn. But the average quotient obtained differs with statistical significance from the quotient in Experiment 5.

Experiment 7. In this experiment, on the other hand, the two points in field I have been readjusted in respect of each other, in such a way that their tracks completely coincide. Thus the points meet and pass one another in the middle of the common track; and subsequently, both simultaneously reach the opposite extreme positions. In order to prevent the possibility of a misleading result taking place due to a recoil effect at the meeting point [14]), one of the objects was raised a very little vertically.

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[14]) When the two objects meet along exactly the same path a phenomenon takes place that is of considerable interest in itself. What occurs can either be conceived of, as two objects that pass each other; or as two objects that collide and rebound. Between these two variants, there are for most of the Os alternations, which cause one to think of alternations in configurations with ambiguous figures.

The recoil effect, that has been mentioned, is effectively, eliminated by means of altering the height, as already described. The recoil effect can, however, be developed so that it becomes inevitable. This can be accomplished in the following manner: There is a vertical fold in the screen just at the meeting point of the objects (⌐¬ profile from above). This is observed from the front, and the fold is then only visible as a faint line; and it is seen from an angle of altitude that deviates somewhat from 90°. As a

Consequently, the two objects do not fully coincide with one another when they meet; for one of them lies about 1 mm. higher than the other. If the two fields are now compared, it will be found that in both cases the distances travelled are the same at the commencement of the experiment, i.e. 20 mm. In field I two objects move in opposite directions along this track; while, as usual, in field II there is only one object.

The results of the experiment are given in Table 7.

TABLE 7

Number of Os	Number of trials	M. II/I	S.D.	S.E.
9	38	2.11	0.42	0.068

It seems that we have a quotient here that exceeds that of the basic experiment. The dispersion in this experiment, as compared with all the previous experiments, is striking. This dispersion prevents the difference between the average quotient obtained here, and the quotient of the basic experiment, from being fully significant from a statistical point of view. The difference is twice as great as its standard error.

An essential cause of the wide dispersion found in this test, is the pronounced deviation from the mean which was noticeable in the case of two of the Os. One O obtained a very high, and

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result of this deviation, to some extent, the objects alter their phenomenal position in altitude when they pass the fold; the consequence is a constant change of identity and a resultant collision.

In this way the two variants can be developed, and an experiment which is carried out with a variant in each of our two fields shows that the phenomenal velocity is distinctly higher when a collision occurs. It can also be pointed out that when the recoil effect takes place, the velocity is no longer experienced as constant; but it is a particularly high just at the moment of collision, and then rapidly diminishes. Consequently the course taken is entirely different from that of our ordinary movements. (cf. page 9).

The recoil phenomenon is an interesting parallel to the causality effects that Michotte established experimentally in his brilliant investigation of causality (A. Michotte: La perception de la causalité, 1946). Recoil affords a pronounced experience of activity and causality, in the same way as did the impacts studied by Michotte. The object recoils because it "impacts".

another a very low, individual mean, and in each case with slight dispersion.

These two extreme cases might, however, prove of distinct interest if an analysis of individual differences in the experience of motion were ever undertaken. The low value is especially pronounced. This O obtained an average of only 1,23. It is possible that we are here dealing with a particular, but consistent observation attitude with a spontaneous keeping apart of the motion of the two elements in field I, i.e. a distinctly analytical attitude. It is of considerable interest that this O was of a pronounced schizothymic type of personality. We may add that a similar phenomenon was observed in connection with another experiment which is not described here. The latter case referred to a distinctly schizothymic type, who apparently had by nature a very analytical observation attitude; and he also obtained a low velocity value for a similar combination of elements.

If we combine the results of the three last experiments, we can establish certain observations which are connected with our own approach to the question. We can state that these experiments clearly and unmistakably, not to say incontrovertibly, refute the assumption that the length of the common track is a decisive factor of equality. We have: *a*) increased the common track in field I (the distance between the exterior turning points). In this case, the more the track was increased, the less was the phenomenal velocity, *b*) the common track was halved in relation to the basic experiment, when an increase in the phenomenal velocity was observed. Neither of these results is compatible with the abovementioned assumption which required a diametrically opposite effect.

The factor which determines the phenomenal velocity in field I, must be sought for elsewhere. To what extent are the experimental results which have now been described, compatible with the third alternative mentioned on page 41. This alternative has still to be considered. In this context, we stated that it was possible to conceive of the experience of the velocity of the two motions uniting to form a kind of whole: a velocity gestalt. This hypothetical velocity gestalt could then have the same value as the sum of the two separate elements.

With reference to the experiments which have just been

described, it is possible to assume that the two objects form a kind of field of forces (if this expression is permissible). The nearer the centre of motion of each element is in relation to the other element's centre, the stronger the bond uniting them will be, and the more intense will our field of forces be.

With the assumption of such a field of forces, the results of Experiments 5—7 would be easier to explain. It can, indeed, be assumed that the direct connection between the tracks of the two objects, as in the basic experiment, is a factor favourable to the attainment of the complete summation of the velocities. The result of this test (1,94) would then lie very close to the ideal quotient 2,00.

By separating the two tracks, as was done in Experiments 5 and 6, on the same basis, the case for the hypothetical field of forces would be weakened. The results obtained in Experiment 5 also showed that the phenomenal velocity had been considerably diminished by separating the two tracks by a distance of 15 mm. Here the quotient was 1,56. And when the difference was 30 mm. (Experiment 6), the quotient was found to be still lower: 1,32. If one were to continue to successively separate the two tracks, then the relative stage would be reached where the quotient had been reduced to 1,00; and consequently no influence between the two velocities would be noticeable. This would signify that the suppositional field of forces had ceased to exist.

If we look at Experiment 7 in this way, where the tracks have been readjusted in the opposite direction, so that they fully coincide; we shall then find that it is just as plausible to assume that this would bring about an intensification of the field of forces in relation to the basic experiment. An assumption that is supported to some extent by the result of the experiment in question.

Hence, the experiments that have been carried out up to the present, seem to furnish us with a working hypothesis. If we are to continue our discussion from this point of departure, we must first undertake an analysis of the preliminary working-conception, "field of forces".

To start with, a foundation should be erected on the basis of which the hypothesis can be discussed and experimentally tested.

We shall commence by returning to a greatly simplified situation, and shall then put the question: In what way do we

experience and estimate the velocity of a single object moving through a homogeneous field? An examination of field II and a thorough phenomenological analysis of the experience, indicate the following: We experience motion as the continuous change of position of an object, primarily in relation to the outer edges of the window. This frame together with its entire surroundings forms a kind of background, an anchorage, or relation basis, for the motion, and acting as a contrast to it. One feels the window to be at rest, it is connected with the observer's own static condition.

Here we have an observed fact that we can safely generalize from: every form of motion is for the perceiving subject the experience of a continuous change of position within a spatial system of relations. This condition has been thoroughly and convincingly studied by Karl Duncker in his investigation of induced motion [15]).

The next stage is more difficult in a way: the experience of the velocity itself; the object of our investigation, though still undefined. From the physicist's point of view, velocity is the time-rate of motion in a given direction. But what does perceived velocity signify for the psychologist? When faced with such a question, and with the physical definition as a background, we have to admit that in a phenomenological study, the unreflecting experience of motion does not afford any measurement in temporal terms. In this, the time-related conception of velocity as found in physics, appears to lack a proper foundation. Velocity appears to be immediately perceptible; *it is a kind of absolute intensity in the motion,* and is just as immediately perceived, as for example, luminous intensity in the experience of illumination.

When experiencing motion, it may prove possible to separate the form and extent of the track, and to perceive it as a spatial pattern at rest. But it is impossible in the same manner to separate the experience of motion and of velocity. The two are really one. The motion of one of our objects is, for the observer, in its periodicity a unit; it has a definite character or profile; and in this, the velocity and the distribution of velocity are inseparably interwoven.

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[15]) Karl Duncker. "Ueber induzierte Bewegung". *Psychol. Forsch.* 12, pp. 180—259 (1929).

It is quite evident that we must take care to distinguish between the experience and the formation of concepts; a distinction which is indeed a fundamental requirement in all phenomenological studies. As experience, velocity in our field of motion, is a kind of intensity; while as a concept, velocity is displacement during a time interval.

This does not in any way contradict the fact that motion-velocity, also in perception, has two dimensions: space and time; and that both of these can be objects of direct experience. In order to apply this to our oscillatorily moving object: we can see directly the length of the track, and we have an immediate experience of the flow of time between different repetitions. But our phenomenological study teaches us that in reality, we cannot experience these three simultaneously. For we either see the whole, the motion gestalt, or we perceive different aspects (space — time). And yet it appears that that which is not in prominence, which has not attracted attention, may be present in the margin of consciousness and from there can determine the actual experience.

Our repeated experimental analyses of the length of the track as the possible equality factor is a consequence of being aware of the ease with which one of those partial factors can suddenly become dominant and can refer the immediate perception of velocity from the focal experience to a kind of perceptual margin, without the perceiving subject actually being conscious that such a change has taken place. We propose to devote the final section of this article to the consideration of the problem of how the two partial factors, from the margin, can determine the focal experience of motion. On account of the previous intimate interconnection of movement and velocity from the phenomenological point of view, it is necessary to point out that when, in the course of phenomenological description, we employ the term "motion", we mean it to refer to every perception of displacement in a system of spatial relations.

The term "velocity" will, on the other hand, be used when it is desired to stress the intensity of this quality.

With this discussion as our background, we can now conduct the experiments which are intended to make our vague hypothesis of a field of forces more concrete.

Experiment 8. As in Experiment 1, there is here only one object in motion in each field. But in field I there is in addition an object at rest in the middle of the track. This stationary object is continually being passed by the one in motion.

The framing of the question which is the actual reason for making the experiment, may be formulated as follows: does an object at rest in the track, exert any influence on the phenomenal velocity of an object in motion through an otherwise homogeneous field?

TABLE 8

Number of Os	Number of trials	M. II/I	S.D.	S.E.
5	20	1.35	0.07	0.016

Experiment 8 gives a definite answer to this question. A considerable increase in velocity is observed in the field in question, when compared with Experiment 1. This result is, in fact, quite surprising; at any rate, when it is a question of the extent of the difference in velocity between the two fields.

Experiment 8 has been varied in different ways. Sometimes, the stationary object was placed so as to coincide with one of the turning points of the moving object, instead of being located in the middle of the track. Also in some cases, two objects were introduced. In one of these experiments, they coincided with both turning points; while in another, they were separated in such a way that they were merely touched by the moving object at its turning points. We can, however, omit these variants here, since their results give no significant difference compared with those obtained in Experiment 8.

As a primary result, we can state that *the phenomenal velocity of an object in motion through a homogeneous field is considerably increased by the mere presence of another object in the same field.*

What does this effect depend on? We connect it with the insight that the experience of motion requires a frame of references for the displacement that motion always signifies; or, as we perhaps should prefer to express it: that every motion entails a change of position within a system of relations. Our experiment affords a very good empirical instance of how this

kind of system of relations can be compressed. For what, from this point of view, does the difference between this experiment and Experiment 1 signify from a purely phenomenological standpoint? That the stationary point in the middle of the track plays, to a very great extent, the role of a frame of reference. This is very easy to demonstrate. One is now almost compelled to see the motion as approaching, passing and receeding in relationship to the adjacent object. This concentration of the relational connection of the motion on a single point has, as shown by the result of the experiment, been *accompanied* by an increase in the velocity. From coexistence, can we infer mutual connection as cause and effect? Does the increase in velocity then *depend on* a relational intensification? We have good cause to answer this question in the affirmative. In the foregoing, we have reached the conclusion that the perception of velocity and the perception of motion from a phenomenological point of view may be one and the same. We also know that the perception of motion is an experience of relations. From this point of view, it is quite plausible to suppose that an intensification of the relational connection, by concentrating on a single point could also bring about a heightening of the experience of velocity. Without as yet, coming to any definite decision, we make this assumption as a preliminary theory by which to explain the increase in velocity.

In all the experiments that have been described up to the present, both the fields consisted of light (translucent) rectangles whose surface was practically homogeneous; (ordinary typing paper was employed as the material for the screen). A few experiments were also made, however, in order to ascertain the possible influence an inhomogeneous field employed as background, might exert on the experience of velocity. For this purpose, squared paper was used as material for the screen. The distance between the lines was 5 mm. The lines were ruled very distinctly with Indian ink, and the squares stood out very sharply and conspicuously.

On the basis of our hypothesis concerning the existence of an organized whole with regard to the perception of velocity, it would be reasonable to assume that such a highly inhomogeneous background would greatly facilitate localization and disjunctive

analysis in a field with two elements in motion; and thus lower the phenomenal velocity there, in comparison with the same motion against a homogeneous background.

No such effect, however, could be detected in connection with the experiments that was made with this inhomogeneous background in both fields. No essential difference took place in the experience of velocity. But even if the velocity is not affected; it seems that the inhomogeneity caused a change in the pattern of the motion; this, however, is a matter that cannot be dealt with here.

On the other hand, an obvious effect relating to the experience of velocity was observed in the following experiment with an inhomogeneous background, which was carried out in connection with Experiment 8.

Experiment 9. Each field only contained a single object in motion. Field I was equipped with the checkered screen. Field II was, as usual, homogeneous, and had the same degree of luminous intensity as field I.

TABLE 9

Number of Os	Number of trials	M. II/I	S.D.	S.E.
5	20	1.29	0.15	0.033

As is shown in Table 9, the experience of velocity is affected to a relatively high degree by inhomogeneity. *An object that moves through an inhomogeneous field has, for an observer, a higher velocity than the same object has when passing through a homogeneous field.*

This corresponds with the results of a similar experiment that J. F. Brown conducted, and where the experimental apparatus was quite different.

The latter had found that an inhomogeneous surrounding field affects the experience of velocity in a positive direction within an inner field. He obtained the value 1,24 for the velocity in the field which had the inhomogeneous surrounding field [16]).

Can the increase in velocity that was observed in this case,

[16]) Op. cit. 1931 pp. 217 f.

be accounted for by the same principle as was adopted for the explanation of the results obtained in Experiment 8?

Apparently the answer to this question is that the experiment does support the theory that has been proposed. What does the intensive inhomogeneity in field I signify here? From the point of view under discussion, it doubtlessly signifies a kind of intensification of the system of reference. The spatial distance from the nearest reference points of the motion has been greatly diminished. Every square is a small field which is traversed; every line is part of a frame of relations which is crossed.

This experiment, however, supplements and extends our theory. For this is not a case of the intensification of the system of reference by means of a relational concentration, as was observed in Experiment 8. The inhomogeneity, as a frame of reference, is just as homogeneous as the original field. No part is more conspicuous than another. Intensification is here a consequence of the diminution of the distance between the moving object and the adjacent reference points. Thus, we have every right to suppose that a spatial proximity factor is active between the moving object and its reference base; furthermore, that the relational intensity is regulated by this factor, and that the percieved velocity [17]) is determined in this way.

It is very likely that this proximity factor was also active in Experiment 8. A study of the variants, as mentioned on p. 50 would probably lead to a confirmation of this. These results certainly indicated that this was probably the fact; but because of the insufficient number of repetitions, the differences are not statistically significant.

In the account given of the preliminary experiment (Experiment 1), it was mentioned that a shutter had been employed, in order to change the character of the event that is percieved. By this means repeated movements were obtained in the same direction, which were separated by dark intervals, instead of being the usual continuous rhythmic to and fro motion. During

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[17]) This can be taken as confirming, in a way, an assumption made by K. Koffka. His hypothesis, supported by deductive reasoning, asserts that the velocity experienced is the slower, the farther the moving object resedes from its nearest relation point. (See K. Koffka, *Principles of Gestalt Psychology*, 1935, p. 290 f.).

this experiment no significant difference was observed between the two variants, as regards numerical results.

The same shutter device was employed in several of the other experiments. (The results of these experiments have not been included in the described material). Here, as opposed to the conditions prevalent in the first experiment, considerable differences were observed between the principal experiment and the variant with the shutter. The latter has all along given a lower value. Thus, the average value of six trials by two trained Os is 1,72 for the same grouping as in Experiment 7, where the result gave a quotient of 2.11. In the same way, in Experiment 8, the result of the 'shutter' variant was distinctly lower (1.21 as against 1.35).

On what does this 'shutter' effect depend? It is evident that the lowered ability to estimate the length of the track, which has already been alluded to, may not be held to account for this; since the effect in question is found to be present both in Experiments 3 and 5, and in 6 and 7.

Also in this case, an explanation could be found by applying the theory that was advanced to account for the influence of the intensity of references on the experience of velocity. Between two objects in oscillatory motion, as in field I in Experiment 3, for example, a very strong relational bond is experienced by the observer: the objects are bound together to form a phenomenal whole; together they form a unitary event. A configuration of this nature has been shown to possess great power of resistance to any attempt made to disintegrate it, as long as the motion itself remains unchanged. Since the shutter breaks the continuity of the motion, it seems that the connection between the two objects in field I is also disrupted to some extent. It is evident that at the very commencement of the light phase in this field, that two disparate elements are visible without any connection due to the motion. It may be assumed that the disappearance of the mutual relational bond during the initial moment, makes the velocity during that moment not being a summation of the partial velocities. The impression of velocity which is given by the field, corresponds, instead, to the velocity of each element taken separately. Although the two elements very quickly begin again to combine to form a configuration, it may perhaps be assumed that the combination only increases in strength succes-

sively. As a result of both the initial disjunction, and the successive increase towards a maximum degree of configuration, it is also possible to assume that the strength of the organized whole of velocity obtained during the light phase, does not attain the same stage as is reached by the continuous motion. The field of forces as a spatio-temporal unit is weakened.

The next step to be undertaken in our analysis is the examination of the basic experiment (Experiment 3). Here, field I contained the highly developed configuration of motion that has been described above. The characteristic feature of this configuration is that the elements mutually bind one another; the relation between them is a kind of coordinative relation, in contrast to the relationship found in Experiment 8, where it was super- and subordinated. Each object is the other's reference base. This intensive mutual anchorage between the objects and the simultaneously, relative weakening of the anchorage of the elements in the background, gives rise to the following effect: the velocity experienced is not foremost the velocity relative to the stationary background (the edges of the window); but the velocity relative to the other object. The common motion, the velocity of the mutual changes in distance, is that which is spontaneously and obligatorily seen and estimated; even if the stationary surroundings do appear to exert a somewhat retarding influence. Consequently, in our particular case, the velocity is doubled in field I; for the velocity of the total mutual changes in the distance are equal to the sum of the individual velocities of the two elements relative to the surroundings.

An objection can be raised against this way of reasoning: we possess an experimental result which appears to contradict it. Our line of argument has been: The velocity of a *single* element in motion is estimated in relation to the stationary background. *Two* objects in motion form a functional organized whole, and their velocity is no longer determined by spatial changes in the relation to the background, but by their relation to one another. But, it may be objected, we have also demonstrated that an inhomogeneous background strengthens the relationship background — object (according to the accepted principle concentration of the relation-heightening of the velocity), and we also know that the inhomogeneity of both fields, as for example, in Experiment 3, did not give rise to any change in the velocity

relationship between the two fields. How can these conflicting facts be reconciled? For example, in Experiment 3, the inhomogeneous background could be expected to increase the velocity in field II, where there is only a single object; but the effect in field I is just the very opposite; consequently an equalization of the velocity relationship (a lowering of the quotient) should take place. In order to be consistent, it must be assumed that the inhomogeneity in field I would bind each object to the background, and thus proportionally diminish the effect of the mutual cohesion. In other words, the possibilities for analysis have been increased. But no such effect has actually been observed.

This objection, however, in the author's opinion, does not constitute an adequate reason for rejecting a theory, that the rest of the experimental results supports so strongly. It may, on the other hand, prove a complementary addition to our theory, if it is considered as follows:

In the relationship motion — background, we have a pronounced super- sub-ordinate relationship; while in the relationship motion — motion, we have a coordinate relationship. Observations show that the latter is altogether much stronger than the former. On the strength of the preceding facts, it may be assumed that the relationship motion — motion has such dominance that the relationship motion — background, even if it is very intensive in itself, it cannot make itself felt when it is in conflict with the former relationship. It is the same dominance that caused the velocity to be doubled in Experiment 3.

Before we finally adopt our theory, however, let us put it to yet another test. In all the experiments that have been described so far, it has all along been the *relative position of the two paths of motion* in field I that was changed. Another method may be employed, however, by means of which it is possible to consider more closely the conditions that determine the complex experience of velocity. Without changing either the grouping of the elements, or their velocity, an attempt may be made to change the *mutual relationships of the velocities of the two elements*. This method of procedure should afford a means of testing the hypothesis concerning the configuration of velocity. If all the factors connected with the elements remain the same as those in an experiment where the velocity (value) is known in

field I; and only the velocity of the mutual displacement between the two elements is changed; and if, in consequence of this, it was found that the velocity value was altered in the same way; then it would surely follow that this relation was really decisive in the experience of velocity. It is just this, that our hypothesis presupposes. In fact, we are to test whether the experience of velocity varies with the mutual relations of the velocities of the elements.

In the earlier experiments, at every moment the two objects in field I have moved in opposite directions in respect of one another. This is the obvious result of there being a phase difference of 180° between the two eccentric discs (they are thus directly contrary), which drive the objects. Between this relation, and the contrary extreme, complete simultaneousness, when the objects continually succeed one another at a constant distance relative to each other, there naturally exist all the possible degrees of dissimilarity or similarity. The more nearly the position of the two eccentric discs approaches a difference of ½ a revolution (180°), the more they will have a contrary motion along the greater part of the track; and the more rapid become the changes in the distance between them. The more the position of the two discs approximates to equality (0° difference), the greater will be the part of the track along which the motion is common; and the slower recede (respectively, approach) the two elements from one another. To summarize, it can be shown that we are able here to vary, at will, between the two extremes, the velocity of the changes in the mutual distance, without in any way altering the physical velocity of the elements. This was accomplished in the experiment we are now about to describe.

Experiment 10. In this experiment the same constellation of tracks was employed as in Experiment 7. Thus the tracks coincided with one another. As the phase difference between the two elements, the mean position between the two above-mentioned extremes was chosen; that is to say, the phase difference was 90°.

Under these experimental conditions, the two elements were seen now abrest, now meeting and now passing. It is difficult to describe the motion clearly. When one element has just passed one turning point, the other is moving through the middle point

of the common track and with the same direction of motion as the former. When the latter element (2) has reached the opposite turning point, the two elements move against one another for an instant, and meet and pass each other along the last quarter of the track. When element 2 has again reached the middle of the track, no. 1 passes the turning point and again begins to follow 2, and meets it on the opposite side of the middle point, and at the same distance from it. The two elements then reach their respective starting points, and a new cycle, which is exactly the same, commences. There are thus two meeting points, and both elements have during one half of the period of revolution the same direction; and during the other half they have the opposite direction.

TABLE 10

Number of Os	Number of trials	M. II/I	S.D.	S.E.
4	16	1.43	0.097	0.024

Table 10 gives the results of the experiment in question. The significance seems unmistakeable, if our formulation of the question is adopted as the point of departure. A comparison between the quotient now obtained (1,43 ±0,024) and the original quotient in Experiment 7 (2,11 ± 0,068) shows at once that the changes that were made in the mutual relations in the velocity of the elements radically affects the experience of velocity which the field in question affords. We have lowered the velocity for the mutual displacement and the consequense has been a corresponding lowering of the experienced velocity. The numerical result also corresponds with our theory to a surprising extent. The relation between the velocity of the elements and the velocity of the mutual displacement are, according to the experimental conditions 1 : 1,50.

The same experiment was made (with only five repetitions) with 135° difference between the eccentric discs. This variant had an average quotient of 1,69, which is entirely in keeping with the results of Experiment 10 (the 'ideal' quotient would have been 1,75).

These results enables us now to make conclusively the definite inference that: *The experience of velocity in our field with two*

objects in motion, is dependent on the reciprocal changes in the distance between the objects, and not on their velocity relative to the background. Having reached this conclusion, we are justified in holding that we have also acquired full confirmation for the rightness of our theory concerning the configuration of velocity as mentioned above.

It now remains for us to also consider Experiments 5, 6, and 7 in relation with the theory in question, and then to study them in the light of those factors which determine velocity, and which we have succeeded in distinguishing.

The combination of motions in these experiments are such, that the doubling of the velocity was to be expected, if only the interplay of the two motions were taken into account. In reality, we found that Experiments 5 en 6 gave a lower velocity value as a result of the paths of motion being separated by 15 and 30 mm. respectively. We represented this as a weakening of the hypothetical field of forces.

If we apply the principle of the dependence of the phenomenal velocity on the spatial proximity of the base of references of the object, we shall also have an opportunity of concreting here. In reality, these experiments taken together afford a very good instance of this principle. The further that the centres of motion of these two objects are removed from each other, the weaker becomes the configuration of the motion formed by them, and at the same time the lower will be the velocity value of the configuration. The retarding effect of the stationary surroundings, may be more pronounced. For the same reason, in Experiment 7, the configuration of velocity is at its maximum. (It even exceeds the theoretical values.) In this experiment, the two centres of motion coincide.

Here a different and very interesting aspect becomes apparent. The last experiments that have been discussed make it highly probable that in the velocity quotient we have a direct quantitative measure of the strength of the cohesion of the configuration of velocity.

We have now completed the account of that part of the investigation, where the length of the track was the velocity variable. There is one point, however, that may be added in conclusion. The velocity in the constant field (field I) has been the same in all the experiments which have been mentioned;

viz. 20 mm/sec. It is apposite to inquire whether the relations between the two fields that have been shown to exist here, also hold good for other initial velocities. Consequently, we wish to point out in this connection, that at a more preliminary stage, the initial velocity has been varied within certain limits, but also in doing so no change in the configurational effect could be discovered.

Hence, the author has considered himself justified in limiting the investigation to a standard velocity. The latter was especially chosen with a view to making the investigation as clear as possible; and also in order to avoid as much as possible causing any unnecessary fatigue.

Before we turn to the next section, we shall give a brief survey of the actual development of the formulation of the theory of the configuration of velocity which the investigation has successively led up to:

Two objects in motion in the same field form a configuration of velocity regardless of whether the field is inhomogeneous or not.

The configuration of velocity is primarily a descriptive aspect of the configuration of motion.

The impression of velocity obtained from a configuration of motion (the configuration of velocity) is, in the first place, estimated according to the changes in the reciprocal distances between the elements, and not by the changes in their distance in relation to the stationary reference points. Besides, the impression of velocity is influenced by, inter alia, the concentration of the reference system, and by the proximity of the reference points.

The relationship motion — motion is very dominant in comparison to the relationship motion — rest (= the subject's own state of motion). This is the explanation of the genesis of the configuration of velocity between two objects in motion.

VI. A Verification of the configurational Character in the Combinations of Motion that were studied

In the study of the configurations of motion which were described in the previous chapter, an interesting suggestion was made regarding the possibility of controling these experimentally. This type of control was actually carried out, and will be

described below. It is based on the following line of reasoning.

If it is assumed that two objects in field I form a functional configuration, then the effect of their motion ought also, in principle, to occur in a configuration, in the more literal sense of the term: a spatial configuration or gestalt, possessing spatial continuity. If it proved possible to procure a spatial configuration, which as regards motion, functioned in the same way as the two points conjointly; and if it could then be demonstrated that the psychological effect was the same in both cases, one of the foundations of the theory would thus be verified beyond doubt. The two separate objects can be considered as the visible terminal points of an otherwise invisible rod which constantly increases and diminishes in length. If this concept is taken to its logical conclusion, and for the object, is substituted a single connected plane, which increases and diminishes also in one dimension; than we clearly have the desired conditions. This was accomplished in the following manner.

Experiment 11. The two fields which are to be compared consist of light rectangles against a dark background. These possess horizontal extension, are 8 mm. high, and of variable length. In field I, both the short sides are movable symmetrically; while in field II, the left side is fixed, and the right is movable. The frequence of the different sides, and the amplitude relationships, correspond directly with the same factors belonging to the objects in experiment 3. Thus, the amplitude of each of the sides in field I is 20 mm. The maximum length of the rectangle is 44 mm. The O can himself regulate the amplitude of the motion in field II.

From a technical point of view, the experimental equipment has been arranged as follows: Immediately in front of the celluloid discs belonging to the apparatus an opaque cardboard screen was placed. The latter has two horizontal slits which are 2 mm. wide. As the projection is 1 : 4; two light bands, 8 mm. wide, are projected on the otherwise dark projection screen. The distance between the slits is such, that the one light band appears in the middle of field I, and the other in field II.

Instead of black points, the celluloid discs had black cardboard bands. These were placed as shown in Fig. 5. As the two cardboard bands that form the short sides of the rectangle in field I

were given the same motion as the objects in the same field in Experiment 3; we thus obtain a symmetrical increasing and diminishing along the horizontal plane in the rectangle; and in field II a similar increasing — diminishing plane was obtained; though in this field the left side was fixed (see Fig. 5).

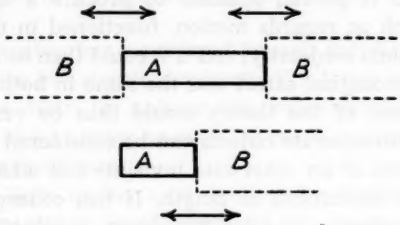

Fig. 5.

O had, in the usual way, to try to establish an equality of velocity between the two fields.

TABLE 11

Number of Os	Number of trials	M. II/I	S.D.	S.E.
3	10	1.84	0.11	0.034

The experimental results are given in Table 11. The quotient obtained was 1,84; and this represents the desired, indisputable verification, when compared with the quotient in Experiment 3 (1,94). The difference between these two mean values is, from the point of view of significance, a borderline case ($0.10 \pm 0{,}043$). Theoretically, a certain difference could be expected. In order to obtain full correspondence as regards the expected velocity the objects in Experiments 3 should not have coincided at their meeting points; but they ought to have had 4 mm.'s free surface between them. A change of this kind would have caused the value of the quotient to fall to some extent when the results of Exp. 5 and 6 is considered.

On the basis of our control experiment, we can assert that there is no essential difference between the experience of the configuration, that the two separate objects form on account of their motion, and the configuration of a coherent surface which increases and decreases.

VII. Frequency (Duration) as a Velocity Variable

It was pointed out on page 33 that with our experimental equipment we can alter the velocity in the variable field in two different ways. The change in the velocity may be effected by altering the length of the path of motion, while the frequency is kept constant; but it is also possible to obtain the same result, by keeping the length of the path of motion constant, and then varying the frequency. Now that we have completed our investigation of the path as the variable; we propose to devote the following section primarily to experiments with the frequency as the variable.

In the former investigations the objective velocity was read, with a high degree of accuracy, directly on a graded scale situated on the lever arm, whose effective length determines the length of the path of the element. This was also controlled by measuring with a vernier caliper, the path of motion of the object which was projected on the screen.

In the following investigations where it is the time that is to be recorded, at a preliminary stage, a stop-watch showing $^1/_{10}$ of a second was employed. It was both started and stopped by the experimenter, when the object whose velocity is to be measured passes a fixed point. The control experiments showed, however, that with this method of procedure, the margin of error was unduly large.

The timing was consequently mechanized in the following manner: The stop-watch was built into a mechanical trigger device which was released by closing the current to an electromagnet. Every such current impulse either started or stopped the watch. Two contact devices, and a three pole switch were inserted in the circuit of the magnet. By means of the switch either one or other of the contact devices could be made to conduct the current, or both could be disconnected.

Since the experimental conditions made it necessary that the velocity of revolution of the eccentric discs that drove the objects to the two fields could be varied quite independently of each other; two separate transmission systems with separate motors were used. On each of the two axles, by which the eccentric discs were driven, a cam was attached. Each of these cams controlled its own contact device; so that the circuit could be

closed for a short fixed time by the rotation of the axles. For a diagram of the apparatus, see Fig. 6.

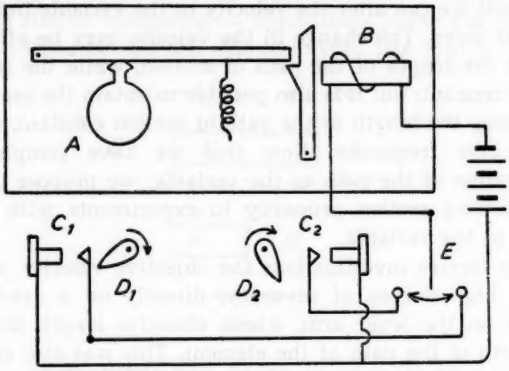

Fig. 6.

This contrivance worked very satisfactorily, as it insured an absolutely exact release of the stopwatch. By noting the time taken for 10 revolutions and dividing the result by 10, an exact result can be expressed in hundredths of a second [18]).

Does a change in velocity due to variations in frequency, give a result that corresponds with that obtained by the preceding method? Does complete parallelism exist between the two methods? These questions were the reason for the following experiments being undertaken:

Experiment 12. This was a simple introductory experiment, where only a qualitative estimate was required. The same combination of movements was employed as in Experiment 7. Firstly, O had to compare the velocity when the track and the frequency were the same for all three objects (i.e. the same as the starting point in Experiment 7), and then he had to make his estimate. Next, the frequency in field II was increased until it became double, and the O had again to compare the two fields, and determine the relationship in velocity.

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[18]) The method is, of course, only applicable, provided that the speed of the motors is sufficiently constant. The control of this is described on pag 33—34.

Six Os took part in the experiment. As was to be expected, all of them stated without any hesitation, that the velocity in field I was much greater during the first phase of the experiment. It was very surprising, however, that the increase in frequency did not make an impression of equality. Just as readily as before, all Os stated that the point in field II now moved much more rapidly than the two points in field I. The author found this result very surprising. What could have been expected was, that equality would also have been experienced here, since the velocity was doubled. Several experiments were conducted in order to study more closely the conditions that gave rise to this result; and also so as to obtain quantitative determinations.

Experiment 13. Firstly, an experiment was carried out that corresponded to Experiment 1 as regards figurative conditions and purpose. Both in field I and in field II there was a *single* object in motion, having a path of 20 mm. The main object of the experiment was to ascertain what degree of precision could be attained when varying the velocity in this way.

It may be assumed that an observer is liable to look for equality in the frequency rather than in the velocity of the motion. Consequently, the instructions encouraged the observers "to try and ignore either equality or inequality in the rhythm and direction of motion; and to estimate the velocity independently of these impressions."

The results are summarized in Table 12.

TABLE 12

Number of Os	Number of trials	M. II/I	S.D.	S.E.
4	16	1.03	0.073	0.018

The adjustment method is employed in the experiments, and the adjusting wheel for the O was coupled to the velocity regulator, that was attached to the motor producing the motion for field II. As shown in the table, the observers were able to acquire equality with a relatively small margin of error. (This experiment does not, however, enable us to draw any other conclusion than that the equality or inequality, which the

observer conceives of as velocity, can be accurately measured by our experimental contrivance).

Experiment 14. This experiment is a quantitative continuation of Experiment 12. Thus, in field I there are two objects whose paths of motion are 20 mm. and which also exactly coincide. Field II contains a single element which likewise has a path of 20 mm. At the commencement of the experiment, field II has a lower frequency. Subsequently, as is usually the case, high and low initial velocities alternated.

The experiment was carried out by 6 Os, three of whom were entirely untrained. Altogether it was repeated 18 times. As shown in Table 13, we find the average equality level, when the ratio is 1,19 : 1,00, between the real velocities of the two fields.

TABLE 13

Number of Os	Number of trials	M. II/I	S.D.	S.E.
6	18	1.19	0.124	0.03

The contrast is striking when we compare the quotient obtained here, with that for Experiment 7. With exactly the same constellation of elements, the latter experiment had a quotient of 2,11. It is clear that when we adopt time as the velocity variable, in stead of the length of the path, entirely different experimental conditions arise. An account of the reflections that this experiment leads to, will be postponed until our material has been somewhat enlarged. Instead, we propose to quote some of the notes in the reports on the experiment, which may prove of some interest.

All the trained observers (3), and also one of the others, pointed out that it was hard for them to obtain a level of velocity in field II which gave an *enduring* impression of equality in comparison with field I. Two of them stated that they found that equality existed at two different velocities in the variable field. When equality had been quite satisfactorily observed, a sudden change might occur which made an obvious correction necessary. These observations may be due to the presence of two contradictory tendencies which were active simultaneously.

Experiment 15. The only difference in the experimental

conditions here, when compared with those in Experiment 14, is that the two objects in field I were separated from each other to such an extent that they only coincided at one of the turning points. Hence, the experiment is a counterpart to the basic experiment (Experiment 3).

The experiment was carried out by three Os and there were 10 repetitions. No significant difference from the result obtained in Experiment 14 was observed.

Here we have come across a phenomenon that is worthy of note. What we can assert with certainty is, that when working with this type of motion, the results obtained in connection with the changes in velocity due to variations in the frequency, are different from the results of variations in the length of the path; even if the velocity measured in mm. per second, is the same in both cases.

The natural consequence of the recognition of this fact is, of course, that we endeavour to bring these two factors in relation to each other under experimental conditions that are not ambiguous. This was the object aimed at in the following experiments. We shall try to proceed in two different ways. We have to try to discover whether it is possible to obtain an impression of equality, by compensating for an inequality in the length of the path, by a change in the frequency. We shall also experiment from the opposite direction and see whether an inequality in the frequency can be compensated, by a change in the length of the path of motion.

Experiment 16. Here we apply the principle that was first mentioned. This is accomplished by only having a single object in each field; but letting the path of motion in the constant field be 20 mm., and 30 mm. in the variable one. The ratio between the two fields, of the length of the path of motion (at the same frequency) and of the velocity is thus II : I = 3 : 2. O has to attempt to obtain equality in the impression of velocity, by altering the frequency in field II.

TABLE 14

Number of Os	Number of trials	M. II/I	S.D.	S.E.
6	18	1.32	0.123	0.029

Table 14 shows the results of 18 trials carried out by 6 Os. The values given in column 3 refer, as in the preceding tables, to quotients II/I of the physical velocity of the two fields. Let us see what this quotient can tell us. Naturally the value 1,00 signifies that Os have adjusted the velocity in such a way that, also the physical velocities of the different objects correspond. This also means that, the frequency ratio II/I equalled 2 : 3. If, on the other hand, the quotient in question was 1,5, this would indicate that Os had experienced equality in the fullest sense, when both fields had the same frequency (but the relation of the velocities II/I = 3 : 2).

The mean quotient obtained in the experiment is, as shown in the table, considerably nearer to the value 1,5 than to the value 1,00. Consequently we have to note a distinct dominance of the temporal factor at the cost of the real velocity. All Os thought that they were comparing the real velocity of motion. In spite of this fact the result of the experiment indicates beyond doubt, that what all Os considered to be an experience of velocity, consisted rather of the experience of a temporal interval, than the experience of a displacement. In other words, the temporal component had been experienced with a relative independence of the motion itself, and it had also been conspicuously dominant in the conflict that arose. This shows that it is impossible, with our experimental contrivance, to compensate for a constant difference in the length of the path, by an inverse difference in the frequency.

In order to study these conflicting conditions more intimately, the following variant of the experiment was introduced:

Experiment 17. This is a repetition of experiment 16, with the one exception that the path of motion in field II was increased from 30 mm to 40 mm. Thus, when the frequencies are equal, the velocity ratio is II/I = 2 : 1; and when the velocities are equal, the frequency ratio is 1 : 2.

TABLE 15

Number of Os	Number of trials	M. II/I	S.D.	S.E.
5	15	1.71	0.14	0.036

The results of the experiment are given in table 15. The

dominancy of the frequency factor is even more marked. The velocity measured for field II has been raised to such a degree, that any idea that the experience of equality could apply to the velocity of the motion, is inconceivable. We have obtained a velocity ratio of 1,71 : 1 for the two fields.

Hence, if Experiments 16 and 17 are compared, it will be seen that an increase in the difference between the length of the paths of motion in the two fields, results in an increased velocity quotient. This leads to an interesting combination. If we relate the value of the quotient obtained for the results of the different experiments, to the figure that represents complete equality of frequence, we shall observe that a surprisingly good constancy exists. This applies to Experiments 16, 17 and 14. In this way, we obtain for Experiment 16, 1,50 : 1,32 = 1,14; and for Experiment 17, 2,00 : 1,71 = 1,17. The corresponding values for Experiment 14 are 1,00 : 1,19. As opposed to the conditions in Experiments 16 and 17, however, a tendency towards complete equality of velocity here will *raise* the quotient [19]). In order to make the relations more easily comparable, we shall invert the relation for Experiment 14, and we then obtain 1,19 as our result.

In this way, the conflict between the two attributes of equality is supported; but above all, the great preponderance of the temporal factor is thus clearly shown. It seems, according to the comparison we have made, that the value which fully covers the equality of frequency has a maximum range of 20 % above or below. All three results lie between 0,1 and 0,2 from this position of equilibrium, and the respective difference between them lacks any statistical significancy.

Consequently, the equality of frequency is decisive for the experience of equality. It has a certain range, or difference threshold. The competing equality of motion press the result toward the borderline of the threshold; but without avail, when the differences in frequency begin to be so large that they are actually visible.

We shall now continue the experiment along the other of the two lines mentioned on page 67.

Experiment 18. Also in this experiment each field contains

[19]) Complete equality of frequency = 1,00; complete equality of phenomenal velocity = 2,11.

a single object; but the two objects have now been given constant frequencies which are dissimilar. The temporal relation between two turns in the two fields is I : 1,50. Field I has the higher frequency. The velocity can be varied in field II, by altering the length of the path of motion.

Thus, O no longer has an opportunity of obtaining either a lower or higher degree of equality of frequency; but the difference in the frequency remains unchanged throughout the experiment.

When the paths of motion were constantly dissimilar; but the frequency was variable, Os endeavoured to obtain equality of frequency (Experiments 16 and 17). It is quite reasonable to ask whether the equality which is spontaneously sought for, is equality of the variable factor. If this assumption were found to be in keeping with the actual conditions, then it is evident that in Experiment 17, we shall obtain a quotient which represents equality in the length of the path of motion. When the frequency ratio is II/I = 2 : 3, this quotient would be 0,67.

TABLE 16

Number of Os	Number of trials	M. II/I	S.D.	S.E.
4	14	1.39	0.097	0.027

Table 16 illustrates the results of the experiment, and also furnishes us with clear and unequivocal information.

That which our Os pursued, was not al equality in the length of the path of motion. The tendency indicated by the quotient (1,39) that was obtained, is of great interest. Not only does the value in question radically differ from the quotient for the equality of the path (0,67): but it also differs essentially from the quotient for the equality of velocity (1.00). Moreover, *it does not lie between these two values* as did the compromise we noticed in Experiments 16 and 17. This fact only admits of one interpretation. The Os have tried to equalize the original difference in velocity. In this way, a phenomenal equality in velocity has appeared; but only when the objective value of equality (1,00) has been definitely crossed.

The slight dispersion, in spite of the number of Os is a convincing testimony that this is a consistent function of ex-

perience. Therefore, we can, without hesitation, generalize the results of the experiment, and hold that a constant difference in frequency: 1 : 1,50, can be compensated for, by an increase of the length of the path of motion; and an apparent experience of the equality of velocity is then observed.

But we can also state something else. The dominance of the temporal factor has again been clearly revealed. Both these experimental methods point to the same conclusion. *It is primarily the temporal factor in an experience of motion which is decisive for the velocity experienced.*

Experiment 18 was varied in the same way as Experiment 16. Thus, a difference in frequency 1 : 2, was introduced. The result of this experiment was almost negative. There was a strong impression that we were here dealing with two incommensurable types of motion. What caused this was that O had increased, without result, the path of motion in field II until it nearly covered the whole field.

We have learned that a shutter which interrupts the continuity of motion in experiments with constant and equal frequency, and a variable path, influenced the velocity quotient in those cases, where this deviated from the value 1,00. It would be very interesting to see how the experiments which we have now been discussing would be affected by a shutter of this kind. For the shutter effect signifies that the continuous duration is, so to speak, cut up into isolated repetitions. It thus influences the dominant temporal factor.

The technical side of the experimental work, however, proved difficult when having to introduce a shutter into an experiment where the frequency is dissimilar in the two fields. Each of the different frequencies of the motion necessitates that the shutter should function in its own rhythm. Consequently, the shutter that had been used previously, could not be employed for these experiments. Its mechanism could not be adapted to the present requirements.

The importance of continuing the experiments along the lines indicated was such, that finally a shutter possessing the necessary qualifications was constructed. It consists of a disc placed immediately in front of the object. The disc is attached to a horizontal arm, which in its turn moves easily about a vertical axle. On the opposite side of the axle the arm was equipped with

a small armature which was in the field of forces of an electromagnet. In this way the shutter was controlled by current impulses to the magnet. Its circuit was closed and broken by two rotating sliding-switches that are attached to the same two axles with which the eccentric discs are connected that control the motion. A special switch conducts the current to each of the two switch devices. This switch is connected with the cord of the projection screen, with which O manipulates the two fields; and this is done in such a way, that the one of the sliding switches is connected up which revolves synchronously with the motion of the objects in the exposed field. Thanks to a damping device the shutter works quite silently. Fig. 7 is a diagram of the apparatus.

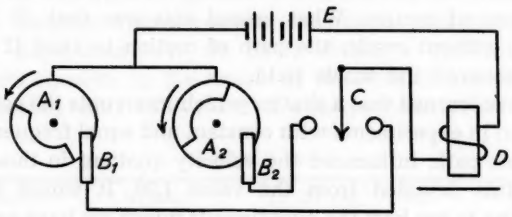

Fig. 7.

This device is so arranged that the object in motion is seen in each of the two fields during the light phase. Every time the light phase commences, the object is in the left side of the field, moving towards the right. The motion is not terminated by a turn; but is cut off, or rather, is switched off at the introduction of the dark phase.

Consequently the motion itself no longer has any frequency of its own. It is almost monotonous, as is a constant, linear motion; it is very difficult to detect pulsation. Instead, we experience a rhythm, which consists of the regular alternations between light and dark phases. The rhythm is thus relatively independent in relation to the motion itself. As a result of this, we can no longer speak of frequency *in the motion;* but rather of its duration.

Experiment 19. This is a repetition of Experiment 16, but with the addition of the double-acting shutter, so that the

pattern of motion changes, as has already been described. Table 17 gives a summary of the results of the experiment.

TABLE 17

Number of Os	Number of trials	M. II/I	S.D.	S.E.
4	20	1.19	0.16	0.036

Experiment 20. Experiment 14 has been repeated here in the same manner. There are two objects in field I, and the paths of motion completely coincide. The results are given in Table 18.

TABLE 18

Number of Os	Number of trials	M. II/I	S.D.	S.E.
3	20	1.35	0.143	0.032

If we compare Tables 17 and 14, we see that the result indicates that the shutter has brought about a lowering of the velocity quotient; i.e. a shift away from the value of an equality of frequency towards equality of velocity [20]). The impression of equality that the equality of frequency signifies, now seems to have partly given way to the equality of objective velocity.

The same tendency is unmistakably shown in Table 18: a shifting from equality of frequency towards phenomenal equality of velocity in accordance with Experiment 7. The quotient obtained here is 1,35, as against a quotient of 1,19 in Table 13. (When the path of motion was variable, and the rhythm equal, the quotient obtained, as has been previously stated, for the phenomenal equality of velocity was 1,72 when the shutter was employed. See page 54).

We can sum up the different points of view regarding our question, which have arisen out of the last two experiments, in the following manner: The separation of the frequency from the motion itself, has not brought about any radical suspension of the dominance of the time factor; but it does signify that the latter has been weakened. The frequency still continues to be active as a factor determining velocity. Instead of the bent

[20]) The difference is approximately 2,8 σ.

towards the attainment of equality in the velocity frequency, as was the case in the preceding experiments, we have now a bent towards equality of duration. A phenomenon that was striking enough when it occured in the earlier experiments, has now become really intriguing. It is not a question of choosing between two possible experiences of velocity; *but an experience of equality can be a compromise between both of these.* Moreover, this compromise takes place, even when the two conflicting alternatives of equality are relatively independent of one another. The results of the last two experiments show that there is a compromise between an equality of motion and an equality of duration which is in relatively free association with it.

The logical outcome of this realization is the question: what influence does a *completely* independent experience of time have on the phenomenal velocity of motion? Judging by the results of the last experiment, the possibility of such an influence cannot be altogether excluded. As a satisfactory answer would be of considerable interest for the discussion of our problem; the author determined to try and obtain this experimentally. He set about achieving it in the following manner:

The oscillatory type of motion which had been used up till then, was now no longer suitable, as the motion was to be freed, to the greatest possible extent, from all time marking.

A continuous rotary motion was now chosen to take its place, where repetitions occur without any definite markings of time intervals. This type of motion could also be produced by the apparatus described above; though now, it was found necessary to utilize one of its functions, that has not been described as yet [21]). The rectangular window in the projecting screen was replaced by a circular window whose diameter was 30 mm. The same objects were used as before, and the diameter of their path was 20 mm. A time marking that was completely independent of the motion was arranged by means of a loudspeaker. The latter was fed by a powerful tone generator, consisting of an electron tube with an oscillating circuit as its oscillator and with a two-

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[21]) The circular movement is obtained by connecting a vertical and a horizontal simple harmonic motion for the same object, buth with a 90° phase difference respectively between those components.

stage transformer-coupled amplifier. A double-action contact device was connected with the loud-speaker circuit by means of the change over switch, which was mentioned on page 72. This contact device revolved at a constant velocity, and thus produced short tone signals in accordance with the following principle. When field I was exposed on the screen, the signals came at 0,8 seconds' interval; and when field II became visible, because of the altered position of the change over switch that was connected to it, the signals were received at 1,2 seconds' intermission. The signals consisted of a single tone with about 800 c/sec. (pitch *g*), and of approximately 0,2 sec. duration. The sound is characterized by a very high intensity. With the aid of this contrivance, the wiring diagram of which is shown in Fig. 8, the following experiment was conducted:

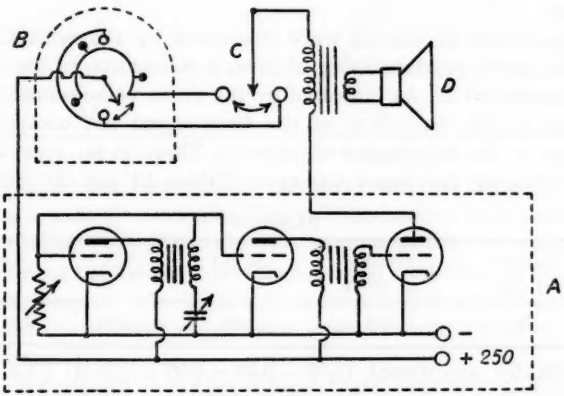

Fig. 8.

Experiment 21. There is a single object in each field. They rotate in opposite directions. The period of revolution is 1,30 sec. in the constant field. The sound signal is not connected. As usual, high and low initial velocities alternate in the variable field. Os are asked to try and obtain equality of velocity in the two fields, by adjusting with the wheel the velocity of revolution in the variable field. The adjusting wheel is connected with the speed regulator of the corresponding motor. The instructions, as usual, warn the observers against attempting to utilize any

secondary criteria; and encourage them without reflection, and quite naively to compare the velocities.

This experiment, as Experiment 1 and 12, aim at testing the ability to attain an equality of velocity. As indicated in Table 19, the precision obtained also in this variant is satisfactory. The result is very similar to that obtained in Experiment 1.

TABLE 19

Number of Os	Number of trials	M. II/I	S.D.	S.E.
7	14	0.99	0.038	0.011

Experiment 22. This is a repetition of Experiment 20, except for the fact that the above-mentioned time marking was employed. The loud-speaker is placed immediately behind the screen.

Experiment 21 and 22 were conducted by the same Os and on the same occasion. Table 20 gives a collocation of the results of Experiment 22. As indicated by the mean value obtained, the change in the frequency of the time signal has occasioned a change in the experience of velocity. There is no great difference between the mean values in Tables 19 and 20; but it is

TABLE 20

Number of Os	Number of trials	M. II/I	S.D.	S.E.
7	39	1.06	0.061	0.01

statistically significant $(1{,}06 - 0{,}99 = 0.07 \pm 0{,}015)$. The result shows us that the lower frequency of the time marking has given rise to a diminution of the phenomenal velocity. In the field where the time signals have a longer interval, Os had to increase the physical velocity, in order to attain an experience of equality. It seems, however, that even here, the velocity experienced was a compromise between two different components of experience. Or perhaps it would be more correct to say, that the time stream flowed more slowly in field II [22]).

[22]) This formulation appeared in an article by J. F. Brown. *"On time perception in movements"*. Cf. J. F. Brown: On Time perception in Visual Movement Fields. *Psychol. Forsch,* 14, (1931).

The material we have obtained, however, indicated that an important difference exists between the last experiment and those preceding it. The coherence of time velocity and motion velocity no longer appears to be general or compelling. For two of our Os, no change in the velocity quotient took place, when the sound signal was connected. In order to illustrate this, the individual mean values are assembled in Table 21. The latter also shows how this "analysts" result has lowered the mean value for the material as a whole.

TABLE 21

Observer	Number of trials	M. II/I	S.D.
W	10	1.10	0.05
R J	5	1.10	0.04
G L	4	1.09	0.09
M O	4	1.09	0.04
S A	4	1.04	0.03
K J	4	1.01	0.03
Li	8	1.00	0.03

In spite of the reservation that these facts occasion, we can state as a general conclusion that the experiment emphasizes, in an intriguing way, the role the experience of time plays in determining the velocity; and that it again confirms the fact that an experience of velocity can be a compromise between, or the resultant of, different components within the total sphere of the experience.

The experiment indicates, however, the necessity of a deeper experimental study of the phenomena observed. A study of this nature, where the experience of time, rather than of velocity, would be the main subject for analysis, is, nevertheless, beyond the scope of the present article.

In the work by A. Dembitz, which was mentioned on page 26, a conclusion is reached that is intimately connected with establishment of the dominance of the time factor which this chapter has led up to. In one of his experiments, Dembitz gets his Os for a short time to watch a motion (a mark on a continuous band), and then to attempt to reproduce the same motion from memory. This experiment was performed at two

different velocities, 16 and 32 cm./sec. Dembitz reached to the following conclusions [23]):

"In relation to the original velocity, for stage I (16 cm./sec.), an error of about 17 % was made; while for stage II (32 cm./sec.), the error was approximately 8 %. Thus, higher velocities are *relatively* more easily memorized than lower velocities; a fact which at first does not seem credible. This, however, leads to the conclusion that the human psyche does not follow the physical definition and dimension of the concept of velocity; does not experience the velocity as cm./sec.; but rather the reciprocal value sec./cm. is valid. Consequently the velocity of movement is not estimated according to the distance travelled in a certain unit of time; but in accordance with the temporal duration required to travel a distance chosen empirically as the unit. Time, duration, thus appears to be the criterion for the psyche when comparing motions and their dimensions."

Demnitz's line of argument seems to be in complete agreement with our findings; and thus constitutes an interesting complement to them. On the basis of our own investigations, however, it must be pointed out that Demnitz, because of the arrangement of the experiment, has made a comparison of 'Dauer', duration, instead of the velocity of motion that was intended. The experiment is in principle a kind of counterpart to our own shutter experiment, with variation of the frequency.

We have now fulfilled the program we had drawn up; in as much as we have succeeded in establishing the fundamental principles which determine the genesis of the configurations of velocity. It is indeed tempting to extend the investigation so as to embrace the experience of velocity on a broader basis. A considerable material for this purpose is already collected. The author has, however, found it expedient in this instance to restrict the work to its present scope; especially as he looks upon this investigation as only constituting a part of the larger undertaking mentioned in the introduction.

Finally, I should like to express my gratitude to professor Katz for the stimulating interest he has shown, and for his co-operation as an observer, as well as for very fruitful discussions of

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[23]) Op. cit. pp. 44 ff.

the problems. I also wish to thank all my other observers for their assistance. I am particularly indebted to my wife and brother who took part in nearly all the experiments, and willingly sacrificed many hours to the performance of an exacting and often monotonous task.

VIII. Summary

The investigation seeks to find an answer to the following two questions: Is the experienced velocity of a visible object in motion influenced by the motion of another object in the same perceptual field? If this is so, what are the factors that determine this influence?

The first question has received a unequivocal affirmative answer experimentally. The experienced velocity can be more than doubled when another moving object is added. Besides, the answer can be extended to apply to the simple presence of an object at rest, and in contact with the path of motion.

The following facts have also been elucidated; and these, taken as a whole, constitute an answer to the second question.

1. Two objects in motion in the same field, form if possible, a single configuration of motion or velocity.

2. Primarily, the velocity experienced, is determined by the respective displacement of the objects from each other; and not by their displacement in relation to the stationary background.

3. The impression of velocity is influenced by the concentration of the spatial relations.

4. Proximity to the reference points is another factor that determines the phenomenal intensity of the velocity.

5. The relationship, motion — motion is very dominant in comparison with the relationship, motion — rest.

6. The perception of velocity is based on the interplay of a spatial and a temporal component. Whenever these are found to be in conflict, then invariably it is the latter which primarily determines the phenomenal velocity.

7. The velocity experienced may be the resultant of two components which are more or less contradictory in their influence.

8. A temporal occurence within the same modality which is only secondarily connected with the motion, can affect the impression of velocity to a great extent. At times, a quite independent temporal occurrence can influence this. In the latter case, an intersensory influence has been observed.

TONE HEIGHT AND TONE CHROMA AS TWO DIFFERENT PITCH QUALITIES *

BY

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Several authors have called attention to the fact that our sensations of pitch consist of two components. One, commonly called tone height, varies closely with the logarithm of the frequency (except for extremely low and high frequencies (1). The other component is best introduced by the terms C-ness, D-ness, etc., as a quality, which contains all its variations within one octave, and then reoccurs in every successive octave (exceptions later). This quality has led to the construction of the piano and organ scale and to the musical nomenclature. (It can be considered as a function of the mantissa of the logarithm to the base 2 of the frequency, and it is independent of the characteristic of the logarithm).

Most authors reserve the name "quality" or "tone quality" for this component of pitch, although the same name could apply equally well to tone height, in contrast to tone intensity. v. Hornbostel calls the periodical quality "Tonigkeit", while M. Meyer suggests the similar name "Tonalität" for tone height, applying the term "Vokalität" to the periodical quality. Since Köhler (erroneously) attributes different vocal (= vowel) characters to different parts of the scale, the adoption of Meyer's term is undesirable. The name "tone color" (Tonfarbe) is used extensively by musicians, but the German term "Klangfarbe" (= sound color) is definitely used to denote the French and English "timbre", and the differentation between tone and sound in English is not as sharp as it is for the corresponding German words "Ton" and "Klang".

In order to introduce a concise, unambiguous term, the

* Other properties of pitch, as postulated by psychologists, are outside of the scope of this investigation.

author (2) has suggested the name "chroma", wich thus far has found application in acoustics only in the word "chromatic scale". The expression "C-chroma" refers to the common aspects of all the C's (C-ness); and the general term "tone chroma" refers to the quality common to all musical tones with the same denomination.

The original distinction between tone height and chroma is based upon subjective experience. Various chromata appear as phenomenal to the musical observer as do colors to the normal individual.

In addition to this phenomenality, Révész (3) quotes several reasons for the distinction of two pitch components. The following seem to be the most important ones:

1. Near the relative threshold of pitch discrimination, quality (= chroma) differences are noticeable, but tone height differentiation is impossible.
2. In the highest and lowest ranges of musical tones, the tone quality (chroma) disappears, while the tone height is still recognized (see later).
3. In a case of "paracusis qualitatis" (reported by Révész) within the range from g_2 to $g\not\#_4$, every tone had the quality (chroma) $g\not\#$, while the tone height advanced normally; therefore, tones in the lower part of the paracustic range were considered as $g\not\#_2$, in the middle part as $g\not\#_3$, and in the highest part as $g\not\#_4$.
4. Many noises have no tone quality (chroma), but they possess tone height.

To these reasons, one may add several more:

5. Tones coming from different sources (violin, flute, singing, whistling, tone variators of different size (2) can be well compared as to their chroma, but a tone height comparison is practically impossible, judgment errors of one or two octaves occurring. In this case the term "sound height" would seem to be more appropriate than "tone height" because of the important role of the overtones.
6. The proper recognition of tone chroma, associated with the failure to recognize the correct tone height, was pointed out by Helmholtz (4): "It is known that Tartini, famous as a violinist and as a musical theorist, determined all the combination tones

one octave too high, while on the other hand, Henrici gives the harmonics of tuning forks one octave too low" (translated).

7. Brentano (5), also distinguishing between the "individual tone colors" (chroma) and tone height, compares the former with actual colors, and the latter with the gray series from black to white. While the "tone colors" are saturated in the middle part of the musical range, they become more and more mixed with black and white toward the lower and higher ranges respectively. Therefore, the tones are recognized better in the middle part than at the extremes. (See later).

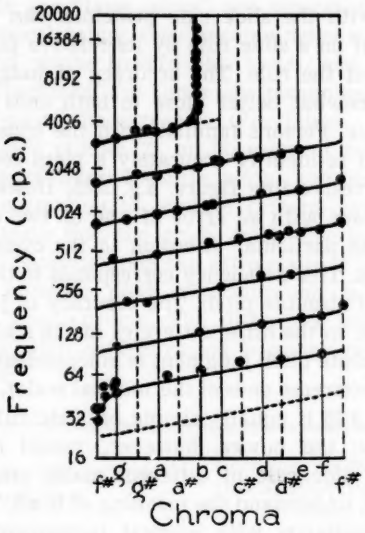
8. The author's studies (2) on 90 cases of genuine absolute pitch, 13 cases of quasi-absolute pitch, and several cases of normal pitch estimation of musical persons (without absolute pitch) demonstrate that the crude pitch discrimination of the normal subject depends upon the estimation of tone height, but that genuine absolute pitch determination is based on the recognition of tone chroma. The difference between the two mechanisms of pitch determination becomes evident from the following chart:

NORMAL PITCH ESTIMATION (BY TONE HEIGHT)	GENUINE ABSOLUTE PITCH DISCRIMINATION (BY TONE CHROMA)
Error distribution follows the general distribution curve.	Error distribution shows sharp maxima at octave distances.
An octave not recognized as such is mistaken for a single tone of a tone height lying between the two components.	An unrecognized octave is mistaken for one of its two compoments.
The average errors of judgment amount to 5-9 semitones (6).	The average errors (notwithstanding the octave errors) amount to 1/16 to 1/5 of a semitone (6).
The accuracy depends much on the timbre of the sound and on the familiarity with the instrument.	The accuracy depends very little on these two factors.
Greatest accuracy occurs for lowest and highest tones of the piano scale.	Accuracy is rather uniform, but declines at both ends of the piano scale.

9. An amazing analogy exists between the pitch scale and the slide rule. Both are logarithmic scales with a cyclic (octave and decimal) subdivision (bases 2 and 10). Normal pitch estimation and genuine absolute pitch discrimination are comparable to two localization methods on the slide rule. Persons, not familiar with the slide rule principle, can remember and identify a point on a slide rule by its relative position between the two ends of the rule. The accuracy of judgment is rather poor; it is somewhat better close to both ends than over the rest of the scale. Persons familiar with the construction of the instrument can re-identify accurately a point on it by remembering the corresponding figure, e.g. 3.35. However, they may identify the place with an error of one or two decimal cycles, not having paid particular attention to the cycle at which the figure was read. This deficiency corresponds to the octave errors of possessors of absolute pitch. The accuracy of judgment is the same, of course, in the different cycles of the slide rule. Correspondingly, absolute pitch judgment is independent of tone height (except for the extreme ends of the musical scale). To the expert, the finding of 3.35 is equally simple on slide rules of different size and shape; the novice, however, would have difficulty working on a slide rule of different make, and the inexpert would not even understand the meaning of it all. This illustrates the role of familiarity with musical instruments of different timbre. It also explains the inability of psychologists (7, 8, 9, 10, 11. 12) to deal with the problem of absolute pitch, if they do not possess absolute pitch themselves, and if they use only non-possessors of absolute pitch as subjects for their research.

10. Several authors (13, 14) recognized that, at the two ends of the musical scale, absolute pitch and successive (not simultaneous) judgments of interval fix the lowest tones too high and the highest tones too low. The detailed study (15) of this phenomenon by this author revealed that pure tones were recognized properly by possessors of absolute pitch up to the frequency of about 4000 c.p.s.; from there on, the chroma of the tones appeared too low with increasing error, until, from about 5000 c.p.s. on, all tones had the same chroma, up to the highest audible frequencies. The usual chroma limit was between B and C$\sharp$. In one case, it was D. In two cases, the deviation started near 3500 c.p.s. and the final limit was G. While the

chroma reached a definite limit, tone height still increased, but in a peculiar manner; increased frequency resulted more in a weakening or peculiar thinning out of tones than in a greater tone height.



The figure demonstrates the chroma advancing periodically with the mantissa (to the base 2) of the frequency and becoming stationary above 5000 c.p.s.

All the observed fourteen cases followed the same general rule; some exhibited slight variations. One subject showed a different chroma limit for each ear in connection with different degrees of presbyacusis. Others demonstrated a less defined limit with a spread over one to two halftones. In several subjects the chroma limit was approached by detour. One of these showed a tonal dip in the region of the detour. In a few cases a multiple approach to the same limit was noted.

Several observations seem to indicate a chroma limitation for lowest audible frequencies. Lack of proper equipment for filtering out overtones prevented further verification.

11. The author has recently described a case of "paracusis qualitatis" associated with a traumatic tonal dip (16). The tonal

dip in the left ear was produced by a shell explosion. Tones just below the center of the tonal dip appeared half a semitone too low; those above the center appeared almost a semitone too high. In both ears tones were heard too high in the deeper region. The latter abnormality was more predominant in the right, less damaged ear. Through this peculiar bilateral paracusis a diplacusis resulted for the highest and deepest acoustic range.

It may be worthwhile to consider the theoretical aspects of the frequency-tone-height-chroma relationship:

The author (2) considers tone (and sound) height as determined by the general position of the whole basilar pattern, probably represented by the center (of gravity) of the disturbance on the basilar membrane. He assumes that chroma perception is due to such fine recognition of details of the basilar pattern, that the octave relationship of tones and sounds through their intra- and extra-aural harmonics can be established. Chroma, therefore, depends upon primary and secondary maxima of the basilar disturbance. The crucial proof for this theory is the author's octave experiment (2). It demonstrates that persons without absolute pitch identify an unrecognized octave by its tone height through localization of the center of the basilar disturbance. It further shows that possessors of genuine absolute pitch identify the unrecognized octave by the chroma of the constituent tones through exact localization of the two maxima of the basilar disturbance. Another fact easily explanable on the basis of this theory is the different tone height (actually sound height) of "tones" of the same chroma from different instruments, i.e., with a different pattern of overtones. The experience finally, that a "noise" (a mixture of very many tones) has no chroma, is due to the fact that its basilar pattern exhibits no recognizable maxima.

The octave periodicity of chroma follows automatically from this theory through the fact that tones in octave relationship contain identical partial tones (through intra- and extra-aural harmonics). It is difficult, however, to account for the constancy of chroma over the highest octaves.

One possibility would be the assumption of 2 different hearing mechanisms: The cochlea for frequency analysis (tone height and chroma), up to 5000 c.p.s.; the saccule for crude hearing, particularly of the highest tones. The belief in the acoustic

function of the saccule in the higher vertebrates, however, rests on weak foundation, although it is widely held (17).

Nor is the application of Luries (18) theory of different functions of the internal and external hair cells feasible for the explanation of chroma fixation, since it is well known that both types of cells end in the same portion of the cochlea (19,20).

The only plausible peripheral explanation of the chroma limit makes use of the observed decrease of sensitivity of the hearing mechanism towards the highest audible frequencies. Since the observed sensitivity to a tone is due to integration of response over a considerable range of the basilar membrane, one must assume an even greater decrease of response for the individual fiber. This assumption is well supported by the predominance of the "tonal dip" in the area around 4000 c.p.s. (21). Assuming that the sensitivity of the Corti organ declines considerably from 4000 c.p.s. on, and realizing that the magnitude of the stimulus falls off rapidly in passing from the oval window through the cochlear canals, one should expect that the 4000 c.p.s. area is the most vulnerable to stimulation. Anatomical evidence supports this assumption of a sensitivity border at 4000 c.p.s. Most authors (22, 23, 24) place the area of the basilar membrane, responsible to a frequency of 4000 c.p.s., at a distance of 9 mm from the vestibular end of the cochlea. This closeness to the fixed end accounts for considerable damping of the basilar membrane, on which even "free vibrations" are somewhat damped by adjoining parts. Alexander's (25) accurate description of the vestibular ending of the Corti organ makes increased damping in that area easily understandable. (Two other factors are responsible for a sensitivity decrease with increasing frequency, the inertia of the conductive mechanism of the middle ear, and the refractory period of the nerve fibers).

The rapid decrease of sensitivity above the 4000 c.p.s. region prevents the maxima of basilar disturbance from moving all the way to the end of the membrane. With increasing frequency, the maxima become stationary and the amplitude of the disturbances declines. These facts explain the observed chroma limit and the observed thinning out of the tones toward highest frequencies. The center of the disturbance may still shift somewhat toward the basilar end up to highest frequencies, and the slight increase of tone height may thus be accounted for.

It is possible to reconcile the offered theory with the finding of Galambos (26) that "specific fibers" exist in the cat for tones of frequencies up to 17,000 c.p.s. which may correspond to 11,000 c.p.s. in man. Galambos' results, compiled in his figure 4, demonstrate the decrease of sensitivity for the cat's ear above 5000 or 6000 c.p.s., corresponding to about 4000 c.p.s. for the human ear.

While in principle a central explanation of chroma fixation seems possible, several observations favor a "peripheral" theory. The multiple approach of the same limit is in agreement with a "harmonic pattern" of disturbance that shifts towards the sensitivity end of the basilar membrane. The different chroma limitation for ears showing different degrees of presbyacusis also points to the internal ear as responsible for the frequency at which chroma fixation takes place. The paracusis qualitatis associated with tonal dip supports the here offered theory.

SUMMARY

1. Pitch consists of two (or perhaps more) components: tone height and chroma.

2. Tone height is a (approximate) logarithmic function of frequency, except outside of the musical scale.

3. Tone height is partly replaced by "thinning out" towards highest frequencies.

4. Tone chroma is an exact cyclic logarithmic function of frequency, represented by the mantissa of the logarithm to the base 2.

5. Tone chroma reaches a definite limit near the upper (and perhaps lower) end of the musical range.

6. Crude (normal) pitch estimation is based upon tone height; genuine absolute pitch judgment is based on chroma.

7. Tone height depends upon the average localization (center of gravity) of the basilar disturbance; chroma depends upon the recognition and comparison of the maximal disturbances.

8. The assumption of a considerable decrease of sensitivity above 4000 c.p.s. explains the observed chroma limit, as well as tonal dip.

9. Tonal dip causes paracusis qualitatis by the same mechanism.

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A COMPARISON OF HUMAN AND ANIMAL LEARNING IN THE STONE MULTIPLE T-MAZE

BY

ØYVIND SKARD, M. A.

INTRODUCTION.

There has been relatively little direct experimental comparison of human and animal learning. The experimental work in this field is rather limited. The main reason for this may be that most learning problems for animals are too easy for the average human, and that it is difficult to make the learning conditions the same for animals and humans.

These two difficulties can be fairly well overcome in the maze learning experiment. The maze is recognized as a fairly good instrument for measuring animal learning. A stylus maze for the blindfolded human subject also seems to be fairly difficult to learn. The data yielded by a maze learning experiment also gives a good basis for statistical analysis.

In the present experiment, 14 white rats learned the Stone multiple T-maze, and 20 students learned a stylus maze of the same pattern and linear relationships. The same learning criteria were used for both humans and animals. The differences which were not eliminated were the following: (a) the different method of locomotion; the rats ran through the maze, while the humans followed a groove with a stylus (b) the rats ran five trials a day until they reached the criterion, while the humans completed the learning in two days with an interval of a week (c) differences in motivation: the motivation of the humans cannot be brought under control to the same degree as the motivation of the rats.

HISTORICAL SURVEY.

Since W. S. Small started to use the Hampton Court maze pattern with animals, a great amount of work on maze learning

experiments is reported. Most of it is on animal learning, but some of it is also on human maze learning. The studies deal with different problems in maze learning, f. the reliability of different maze patterns, the different cues utilized in maze learning, whether number of trials, number of errors, or learning time is the best measure to be used, the order of elimination of blind alleys, motivation, transfer of training etc.

One of the earlier studies reported of a direct comparison of human and animal learning in a maze is that by Hicks and Carr (3), in 1912. They used a different maze pattern for the humans and the animals. The animals used the Hampton Court maze, and the humans used a full size maze of a different pattern. The authors state that "any comparison of the relative difficulty and complexity of the two mazes is a matter of opinion". They also used a different criterion of learning for the humans and the rats, one perfect trial was required of the humans, while two perfect trials in succession were used as criterion for the rats. Very little difference in the number of trials to learn was found. The authors do not advance any definite conclusions because of the different maze patterns and the different criteria employed.

L. W. Webb (10) in 1917 reported a study of transfer of training and retroaction. He used several different maze patterns, and used both animals and human subjects; the humans had to learn stylus mazes of the same patterns as the animal mazes. Webb's main objective was not a comparison of human and animal learning. But he reports that the transfer effect from one maze to another is positive and about the same for rats and humans.

In a study of the reliability of mazes W. T. Heron (1) and W. S. Hunter (4) found a greater reliability of the maze for human subjects than for animals. In a study of habit interference Hunter (5) in 1922 used a circular maze for the rats and a pencil maze for the humans. The pencil maze was a different pattern. He first let both the humans and the rats learn the mazes. Then he changed the sequence of the turns in the maze to the opposite, and let the two groups learn this new pattern. He found that human subjects in a pencil maze gave a higher correlation than the rats in an animal maze between forming one habit and replacing it with its opposite.

Richard W. Husband (6) made a comparison of human and animal learning with the same maze pattern for both groups.

The maze pattern he used was the Warden U-type maze, a high relief finger maze for the humans and an enclosed alley maze for the rats. His results showed that humans needed only about $^2/_3$ as many trials to learn as the rats, but that rats were comparatively less variable. Entirely different errors were made by the two groups. The humans learned the outside portions first, the rats learned in a regressive order. The learning curves were very similar in shape, the curve for humans always below that for the rats. Like Heron (1) and Hunter (4) he also found a greater reliability of the maze for the human group than for the rats.

The same maze that was used in Husband's experiment was also used in a similar experiment reported by Clark Lathan and Paul E. Fields (7). In this experiment an effort was made to make all the physical conditions as equal as possible. The same maze pattern was used for both humans and animals, preliminary orientation was attained for both groups by use of a straightway, the number of trials per day was constant, and learning criteria were identical. Dissimilarities were: (a) motivation not equated, (b) the humans walked on the floor and followed the pathway with the hand, while the rats walked along an elevated path, (c) the humans were blindfolded. In contradiction to Husband's results, it was found that the rats were superior to the humans on ten of the twelve different criteria employed. In agreement with Husband's study, it was found that the rats were in general less variable than the humans. Like in Husband's study, it was also found that different types of errors were made by the two groups, and that the rats learned the last section of the maze first. As regards the reliabilities computed through errors and time on odd-even trials (with first two trials omitted), they were found to be higher for the rats than for the humans. This does not agree with the results of most earlier studies.

According to the different studies made, there seems to be fairly good agreement that the rats are less variable than the humans, and that human subjects and rats make different kinds of errors. The studies made so far have come to different results as regards whether rats or humans are superior in maze learning, and also as regards the question of whether the maze is most reliable for humans or for rats.

APPARATUS AND METHOD.

The maze pattern used for both humans and animals in the present experiment was the Stone multiple T-maze pattern.

The animal maze.

The maze used by the animals is the same as described by W. T. Heron (2). Fig. 1 shows the floor plan of it.

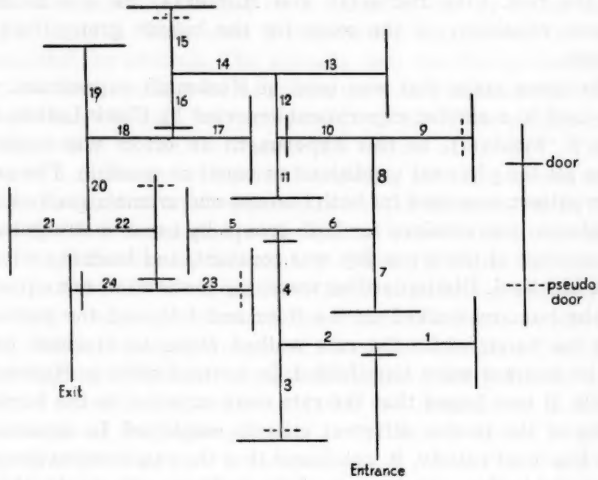

Fig. 1

The animal maze was of the usual enclosed alley type. The maze rested upon a big table covered with linoleum. The table was not washed every day. But it is not very likely that the odours served as an important cue for the rats, as they left their odours in all parts of the maze. From their behaviour they seemed to take their cues more from the sides of the maze than from the floor.

The numbering of the alleys is the same as used by Stone. To prevent retracing, doors of screen wire were let down after the rats had passed, in the six places indicated on fig. 1. When raised, these doors were pressed flatly against the screen wire covering all the alleys. In order to eliminate these doors as cues for the rats, dummy doors are inserted at the opposite end of

each alley with a real door, so that the two ends of an alley always look exactly the same to the rat.

The human maze.

The maze used for the humans was a stylus maze of exactly the same pattern as the animal maze. The linear relationships were the same. A groove of ¼ inch width and approx. ¼ inch depth had been made in a plastic plate. The sides of the groove were very smooth and could not give any cues. The bottom could give no cues either, as the grooves had been made by making the alleys go through the whole thickness of one plastic plate, and then this plate had been fixed on top of another plate. Fig. 2 will show this clearly.

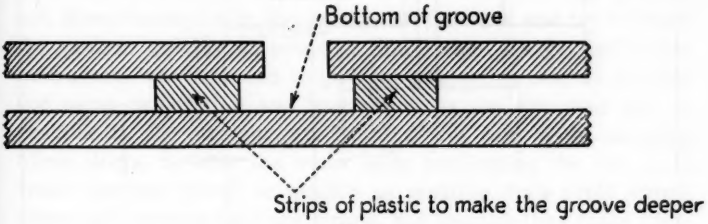

Fig. 2
Cross-section of an alley in the human maze

Instead of the doors in the animal maze small pieces of metal were used. These closed the grooves behind the subject when the experimenter turned or pulled a knob. They could be worked without making any noise. As the human subjects were blindfolded, the question of visual cues did not enter in. The position and construction of the doors in the human maze is seen on figure 3.

The numbering of the alleys was the same as on the animal maze. The subjects used a blunt wooden stylus to follow the grooves.

Subjects.

As subjects were used 14 white rats, who were selectively bred bright maze runners. Their age at the time of the experiment was about 3½ month. The human subjects were 20 university students, age range 21 to 27, with about the same educational

background. None of the human subjects had any experience in maze learning experiments.

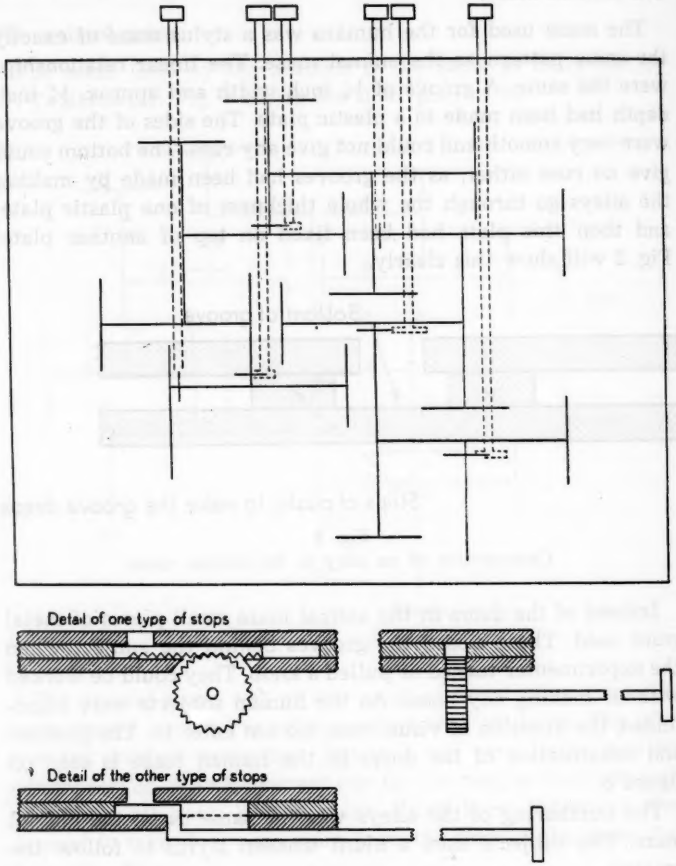

Fig. 3

Method.

The rats. Before the experiment was started the rats had one trial each day for ten days in a straightway. They were fed only after they had run through the straightway. The training

was started at 11 a.m. each day, and so was the experiment itself.

When the experiment itself was started, each rat had to run the maze five times a day. When a rat came to the food box at the end of the maze, it was picked up almost immediately and placed at the starting point again. After the rats had finished five trials, they were put in separate feeding-cages and allowed to eat exactly the same amount of food every day.

The human subjects did not get any previous training. They were blindfolded, placed on a chair in front of the stylus maze and given the wooden stylus. The experimenter then took the subject's hand, placed the stylus in the groove, and gave the following instruction: "Here is the starting point. You are to slide the stylus along in the grooves until I tell you that you are out. Keep the stylus in the grooves all the time and try to reach the goal as quickly as possible. Start when I say "Go"." This instruction was supposed to give the subject as near as possible the same orientation and motivation as the rats had got by practice in the straightway. Nothing was said about avoiding blind alleys, because the main thing motivating the rats is to reach the goal (food) as quickly as possible. Rats avoid wrong alleys not because they are wrong, but because they make them reach the goal later. The human subjects were not allowed to let hand or arm rest on or touch the maze. Nothing was said about the results till S. had reached the criterion for learning and finished the experiment.

The human subjects finished the learning in two days with an interval of one week.

The criterion for learning was for both groups two perfect out of three trials. An error was for both groups any move which led into a blind alley or any move backward in the true path. A backward move was counted as one error only until a movement forward again was begun.

RESULTS.

1. *Trials to learn.*

In table 1 will be seen the mean number of trials necessary for the two groups to reach the criterion of learning.

TABLE 1.
Trials to learn

	Mean	Range
Humans	27.05	11–69
Rats	37.5	22–62

It will be seen that the humans needed about $^2/_3$ of the number of trials necessary for the rats. The variation in the human group is much greater than in the animal group. An F test showed an insignificant difference in variability between the two groups. (The F value on the 1 % level is 3.67, on the 5 % level it is 2.46. The F-value in the present case was 1.798). A T-test of the difference in means was therefore made, and gave a t-value of 1.855, with an approximate probability of 0.81, showing that the difference in means is not statistically significant.

2. *Number of errors.*

Table 2 will show the mean number of errors made by both groups.

TABLE 2.
Errors to learn

	Mean	Range
Humans	67.20	20–150
Rats	161.07	108–204

This also shows the humans with a much lower mean number of errors than the rats. Again it will be noticed that the range is wider in the human group than in the animal group. An F-test gave a result of 2.461, showing a difference in variability which is significant on the 1 % level. A T-test for the difference in the means is therefore not statistically justifiable.

3. *Learning curve.*

The learning curve in fig. 4 shows the average number of errors per trial for each subject in the first five trials, the second five trials etc.

The rats have a higher average number of errors from the beginning and till the 35th trial. After the 35th trial the two groups are about equal as regards average number of errors per trial.

The learning curve for the time per trial was not made up, as it was of no great interest in an experiment on a comparison of human and animal learning where the method of locomotion and the size of the mazes used is so different for the two groups.

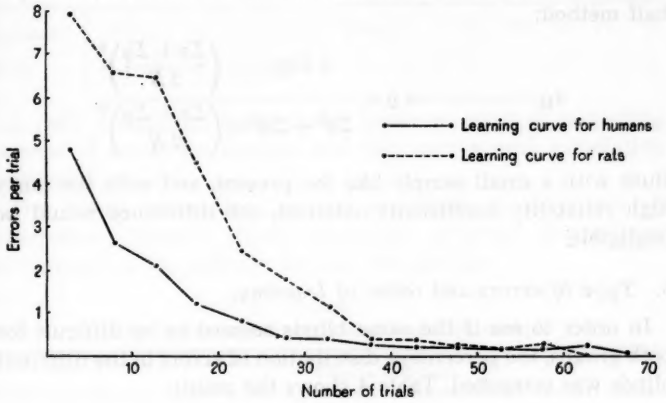

Fig. 4

4. *Reliabilities.*

The reliability of the maze in the present experiment was computed for both the human and the animal group. Both the odd even reliability of the time measurement and of the errors made were calculated. Some earlier investigators have used the odd even reliabilities for time and error measurement with the two first trials excluded, so these reliability coefficients were also computed in the present experiment. The different reliability coefficients obtained are shown in table 3.

TABLE 3.
Reliability coefficients

	Humans		Rats	
	Prod. mom.	Corrected	Prod. mom.	Corrected
Total errors	.9608	.9800 ± ,0045	.6789	.8087 ± .0565
Errors, two first trials out	.9535	.9762 ± .0053	.7958	.8863 ± .0322
Total time	.9389	.9685 ± .0071	.9009	.9479 ± .0144
Time, two first trials out	.9778	.9888 ± .0025	.9081	.9518 ± .0132

The ordinary product moment method was used, and the obtained correlation coefficients were corrected for halving the data by Spearman Brown prophecy formula. The writer is aware that the most correct formula to be used in split half correlation is the formula for the maximum likelihood estimate of the split half method:

$$r_{11} \longrightarrow \varrho = \frac{2\,\Sigma xy - \left(\frac{\Sigma x + \Sigma y}{2N}\right)^2}{\Sigma x^2 + \Sigma y^2 - \left(\frac{\Sigma x + \Sigma y}{2N}\right)^2}.$$

Buth with a small sample like the present, and with the fairly high reliability coefficients obtained, the difference would be negligible.

5. *Type of errors and order of learning.*

In order to see if the same blinds seemed to be difficult for both groups, the percentage distribution of errors in the different blinds was computed. Table 4 shows the result.

TABLE 4
Percentage distribution of errors in the different blinds.

Blind no.	1	3	5	7	9	11	13	15	17	19	21	23
Direct. of choice	H	V	V	H	H	V	H	H	V	H	H	V
Humans	6.95	3.51	26.03	3.82	2.52	7.94	2.90	13.51	16.41	3.97	8.47	3.97
Rats	15.67	8.91	10.28	8.72	12.88	3.38	4.80	9.0	3.52	4.43	11.24	7.17
	1st section				2nd section				3rd section			

The distribution of errors in the first, middle and third section of the maze is shown in table 5.

TABLE 5
Distribution of errors.

	Humans	Rats
	percent	percent
First section	40.31	43.58
Middle section	26.87	30.06
Third section	32.82	26.36

Table 6 shows the blinds in order of increasing percent of errors for both groups.

TABLE 6
Blinds in order of increasing number of errors.

	Blinds number											
Humans	9	13	3	7	19(23)	23(19)	1	11	21	15	17	5
Rats	11	17	19	13	23	7	3	15	5	21	9	1

A rank correlation between the blinds in order of increasing number of errors for humans and rats gave a rank correlation coefficient of –.047.

The blinds were also grouped according to the order of elimination. Table 7 shows when the different alleys were eliminated on the average for the two groups.

TABLE 7
Blinds in order from first to last eliminated.

	Blinds number											
Humans	13	9	7	19	3	23	11	1	21	15	5	17
Rats	19	13	11	23	17	15	7	9	21	5	1	3

A rank correlation between the order of elimination for the two groups gave a correlation of .28.

The rank correlation between the blinds in order of increasing percent of errors and blinds in order from first to last eliminated gave the following rank correlations:

For humans: .959
For rats: .778

DISCUSSION.

1. *Trials to learn.*

The difference in the mean number of trials to learn corresponds with Husband's results (5), which were that the humans needed about $^2/_3$ of the number of trials necessary for the rats. The situation is of course not quite comparable for the two groups of subjects. In the writer's opinion the main difference is the different method of locomotion, which favoured the human

subjects with a much better spatial orientation (in relation to their own body). Their body served as a point of orientation all the time. After the first trial the humans would very likely have a clearer image of the general direction of the true path than the rats would have. It would very likely be difficult for the rats to have any clear orientation in relation to the starting point during the first trials after they had been in each blind alley several times. On the other hand the animals could use their vision. The maze did not give them any visual cues, however, and from every point of choice both alleys looked exactly the same. The human subjects did not know where the choice points were. Several of the subjects said during the experiment: "I know I am in a wrong alley, but I do not know where the choice point is where I choose wrong."

In the writer's opinion, the difference in method of locomotion and consequently in degree of spatial orientation for the two groups is so great that the mean number of trials to learn for the two groups is not very useful, and no definite conclusions can be drawn on the basis of the mean number of trials to learn as regards a comparison of the learning ability of the two groups.

Much more interesting and important are the differences in variation between the two groups. The F-test gave an insignificant difference in variation. But the difference is relatively large. The range for the animal group was from 22 to 62 trials to learn, while some of the humans needed much fewer trials to learn than the best rats (lower range limit was 11), and some of the humans needed more trials to learn than the most slow-learning rats (upper range limit was 69). It has been mentioned by earlier investigators that one of the reasons for the small variation in the rat group compared with the humans might be that selected bright maze-runners were used in the experiment. This was also the case in the present experiment. At the same time one might argue that the human group was a selected group too, since they were all students with several years of successful university work. But as the correlation between academic achievement and maze learning is not very high, the groups may not be equal as regards selection.

The difference in variability between the two groups may be because of differences in the methods of learning. The rats probably have not so many different methods of learning as the

humans. Their learning will mainly be characterized by trial and error. The humans presumably use intellectual devices which are beyond the rats. These intellectual devices sometimes are a help in learning, and sometimes they slow up the learning process. The human ability to verbalize as a help in memorizing will be a help in the maze learning. The humans have different ideas of what the maze is like and of what is the best way of learning it. Some of these ideas may be a help in the learning, but some of them make the learning more difficult. After the present experiment three of the human subjects reported that they went into a particular blind alley because they thought they had to go in there in order to find the parallel alley which they had to follow out again. Some reported that they in the first trials went into almost every blind alley on purpose, because they wanted to know the maze. Others tried to go through the maze as quickly as possible. Some became rather desperate and emotionally upset during the first trials while they were wandering round in the different blind alleys having lost completely any plan they might have made up. The great difference in variability may therefore perhaps be taken to indicate the greater richness in methods of learning and intellectual devices used by the human group.

2. *Number of errors.*

In the mean number of errors made by the two groups in order to learn we see the same tendency as in the mean number of trials, only the difference is greater. The difference in variance was significant on the 1 % level, so no t-test was made of the difference in the means. The tendency for the humans to make fewer errors per trial on the average is caused mainly by the much lower number of errors in the first trials (see discussion of the learning curve). From the data it seems as if some of the humans could do only one error over and over again for a much longer time than the rats. Two of the human subjects made only one and the same error for 20 and 30 trials before they finally learned the true path. The reason for this may have been that the human subjects have had some idea of what the maze was like or some method of learning which made the learning process more difficult.

3. *Learning curve.*

The learning curves presented in fig. 4 are very similar in shape. The curve for the rats is above the curve for the humans until the 35th trial. After this the curves follow each other fairly closely. This shows that the humans have an advantage in the earlier part of the learning process, possibly because of a better spatial orientation and a better memorization than the rats. The closeness of the curves after the 35th trial shows that it was just as difficult for the humans who had not yet reached the criterion for learning to eliminate the most difficult blind alleys as it was for the rats. (After the 35th trial there were 4 humans and 6 rats still running the maze).

4. *Reliabilities.*

It will be seen from table 3 that the human group in general tends to be more reliable in the maze learning experiment than the rats. This corresponds with the findings of Heron (1), Hunter (4) and Husband (6). The correlation between the errors on odd and on even trials and between the time on odd and on even trials is about the same for humans. For the rats the correlation between the time on odd and on even trials seems to be greater than the correlation between errors on odd and even trials. For the rats there also is a tendency toward a higher reliability when the two first trials are cut out. This might be explained by the unproportionately long time the rats in general spend on the first trial, when they usually go into every blind alley and "explore" the maze. After the first trial, when they have found the food box, they are much stronger motivated to get through the maze quickly. But no definite conclusions can be drawn, as none of the differences in reliability within each group is statistically significant. The difference between the reliability coefficients for the errors on odd and on even trials for the humans and for the rats is significant however.

The difference between the reliability coefficients for the time on odd and on even trials for humans and for rats is not significantly different from each other.

The changes in reliability coefficients for the human group by leaving out the two first trials are very small. This might be because the change in the motivation when they have reached the goal once is not very great for the humans.

In general, the correlation coefficients obtained are about the same as reported by Stone and Nyswander (8) for the rats.

5. *Type of errors and order of learning.*

From table four it will be seen that the errors made by the two groups seem to be very different from each other. F. inst. alley 1, 9 and 21 had for the rats 15,67 %, 12.88 % and 11.24 % of all the errors, while they for the human group only had 6.95 %, 2.52 % and 8.47 %. A rank correlation between the blinds in order of increasing percent of errors for the two groups showed no association between the two groups as regards this ranking ($r = -.047$). There was only a very small rank correlation between the order of elimination of the alleys for the two groups.

It has been very common among earlier investigators to divide the maze in a first section, midde section and a final section, and to see which part of the maze as a whole was most difficult or was eliminated first. By doing this, we get in the present experiment the usual result for the rats, with most errors in the first section and least errors in the last. But if one examines the distribution of errors within the last section, (alleys 17, 19, 21, 23) one will notice that the goal gradient is not very clear. The next to the last alley (21) is among the most difficult ones, and has a percentage of errors of 11.24. This alley is also among the last ones to be eliminated. One will also notice that in the middle section is the alley with the smallest number of errors (11), and two of the alleys in the middle section (13 and 11) are among the first alleys eliminated. This indicates that there are other factors influencing the distribution of errors and order of elimination than only in which part of the maze the blind alley is located.

When we look at the distribution of the errors of the human group, we see that it is very irregular. Warden (9) found that the beginning and end is mastered before the middle by blindfolded human subjects. This is not the case in the present experiment. The humans had most errors in the first section of the maze, and the smallest number of errors was in the middle section. We also see from table 7 that two of the alleys in the middle section (the supposedly most difficult part of the maze) are eliminated first of all blind alleys (13 and 9).

If we look at the distribution of errors within each section, we find the same irregularities that we found for the animal group. We will f. i. see that the reason why the first section has most errors is that one alley in the first section has the greatest number of errors in the whole maze (blind no. 5). And here too we find in every section one or two alleys with a great number of errors, and the others with a fairly small number of errors. This again seems to point to other factors than the mere numerical order of the alleys in the maze. The spatial position, and perhaps the sequence of turns coming in front of each individual alley may have some influence. For the human subject, the position and direction of the alleys in relation to the subject's body, and the special technique of tracing the alleys with a stylus may also enter in as influencing factors.

The blind alleys with most errors were for the humans alleys number 5, 15, and 17, for the rats alleys 1, 9, and 21. The blind alleys with the smallest number of errors were for the humans alleys number 3, 9, and 13, for the rats alleys number 11, 17 and 19. So the difficulties seem to be of different kinds. If one analyzes the position of the alleys with most errors for the humans, one will notice that both alley 5 and alley 15 comes after a sequence of alternate turns, alley 5 after a turn sequence of L R L, and alley 15 after a sequence of L R L R. It will also be noticed that blind alley 21, which comes after a similar sequence of turns (L R L R) has the largest number of errors after alleys 5, 15, and 17. This might point to a tendency in the human subjects to choose alternate turns. That this tendency does not influence the number of errors in alley 23, which comes after a sequenze of L R L, is explained by the fact that alley 23 is so near the end of the maze that this turn is easily memorized. The reason for the great number of errors in alley number 17 cannot be explained in this way. May be there is a tendency for the subject to keep fairly close to the midline of the maze. The fact that alley 5 has the greatest number of errors of all the alleys for the humans might be explained by the tendency to choose alternate turns and the tendency to keep fairly close to the midline of the maze working together. Apart from alley 23, which is easily memorized, alley 5 is the only blind alley where these two tendencies work together.

The number of errors in alleys 9 and 21 is not as big as one

might expect, even if they both come after a sequence of turns of R L R. But this might be explained by a conflicting tendency for the subjects not to go too far to the sides of the maze.

Of the blind alleys with the fewest number of errors for the humans, both alley 3 and 13 come after two turns to the same side (L L and R R). This again supports the theory of a tendency for the humans to take alternate turns.

It is interesting to see that two of the alleys with most errors for the rats also come after a sequence of R L R (alleys 9 and 21). The rats could not have the same feeling of going far out to the side of the maze in relation to their own body, a feeling which may have prevented the human subjects from taking sequences of turns which they otherwise would be apt to take. The errors in alley 21 may also be explained as an anticipation of alley 24 leading to the goal.

When we study the blind alleys where the rats made the smallest number of errors, it is noticed that all of them (blinds 11, 17 and 19) come after two turns to the same side. (L L, L L, and R R). And none of the other alleys coming after a similar sequence of turns have a great number of errors. If the rats have a tendency to avoid a sequence of turns to the same side, this would make them go into blind alley 23 very often. And every rat went into alley 23 till about the tenth or fifteenth trial, then it was eliminated because it is so near to the goal. The only other blind alleys where one would expect a great number of errors for the rats because of a tendency for alternate turns would be blind alley 5 and 15. And these two alleys have the greatest number of errors after alley 1, 9, and 21.

The present data seem to give fairly good support to the theory that both human subjects and rats have a tendency to choose alternate turns. For the humans this tendency is checked by the spatial relation of the different parts of the maze to their own body, which makes them avoid alleys going far out to the sides of the maze, and keep fairly close to the midline of the maze.

Observations

Observations in an experiment like this cannot prove anything. But they may perhaps give support to some of the theories based on the quantitative data, and they may give some additional information.

The humans seemed all to be working according to some plan. This was also reported by most of them. Sometimes if they went wrong in one alley, they seemed to get completely lost, and evidently trying to find the path completely at random, in a trial and error fashion. It gave the impression that one error might break up the whole working plan, and that no new plan suggested itself immediately. Errors that had been avoided many times before were done again when the subject was in this situation.

The behavior of the rats could not be seen to be influenced by any special plan. Their behavior during the first trials was very much like the behaviour of the human subjects when the humans' plan "did not work" and they got lost in the maze.

SUMMARY AND CONCLUSIONS.

1. Fourteen white rats and twenty students learned the Stone multiple T-maze, the rats using an enclosed alley maze, the humans using a stylus maze. Both groups were stopped from retracing by closing the alleys in six different places. The main differences between the learning problems for the two groups were: (a) the rats had to run through the maze, while the humans traced a small sized maze with a stylus. This gave the humans a different spatial orientation toward the different parts of the maze than the spatial orientation of the rats; (b) the rats learned the maze by five trials a day, while the humans learned the maze in two days with an interval of a week; (c) the humans received verbal directions when they started, the rats had ten days' training in a straightway, one trial a day.

2. The human group needed 27.5 trials to reach the criterion of learning, the animal group needed 37.5. The difference is great but not statistically significant. Because of the differences in method of locomotion and in orientation, the difference cannot be made the basis of any definite conclusions.

The mean number of errors before reaching the criterion was for the human group 67.2, for the rats 161.07. Because of above mentioned reasons this cannot be made basis of definite conclusions. These results check with the results reported by Husband (5).

3. The difference in variability between the two groups is large. The difference in variability as regards trials to learn is

not statistically significant. Range for humans was 11 to 69, for rats 22 to 62. Difference in variability as regards errors to learn is statistically significant. Range of humans was 20 to 150, animal range was 108 to 204. This shows a greater variability in the human group than in the animal group. The results check with results reported by other investigators.

4. The learning curves are similar in shape, the humans having a much smaller number of errors per trial in the first half of the learning process than the rats.

5. The reliabilities reported were computed by correlating errors on odd and even trials, and time on odd and even trials, both with and without the two first trials. The human group in general tends to give higher reliabilities than the animal group. The difference between the reliabilities obtained by the correlation of errors on odd and on even trials for the human and the animal group is statistically significant. The reliabilities of the time measurement and error measurement is about the same for the humans, but for the rats the time on odd and even trials give higher reliability than the errors. The rats also give higher reliability when the two first trials are excluded. But none of the differences between the reliability coefficients within the groups are statistically significant.

6. The rats have most errors in the first section and fewest in the last section. This corresponds with most other reports. But the humans have most errors in the first section and fewest in the middle section. This is in contradiction to the results reported by Warden (9). By analysis of the errors in the individual blinds, there is fairly strong evidence of a tendency both for the human and the animal group to choose alternate turns, and to avoid consecutive turns to the same side. This tendency is for the humans interfered with by the tendency not to go far out from the median line of the maze.

SUMMARY.

Fourteen rats were run in the Stone multiple T-maze, and twenty blindfolded students learned the same maze pattern as a stylus maze. The rats learned the maze by five trials a day, the students learned in two days with an interval of one week. The students needed on the average 2/3 of the number of trials necessary for the rats to learn. No definite conclusions are drawn from this because of the different technique employed by the

two groups. Like in similar studies the variation was greater in the human group than in the animal group. The odd even reliabilities were obtained for time on odd and even trials, and errors on odd and even trials, both with and without the two first trials. The reliabilities are in general highest in the human group. For the rats, the reliability found by correlating the time on odd and on even trials was higher than when the errors were correlated. As in most experiments on animal maze learning, the rats had fewest errors in the last section, and most in the first. The humans, however, had most errors in the first and fewest in the middle section. The distribution of errors within each section is irregular, usually one alley in each section with a great percent of errors and the others with rather few. This is taken to indicate that other factors than the goal gradient is at work. By analysis of the errors in the individual alleys, the author shows how both humans and rats seem to have a tendency to prefer alternate turns, and avoid consecutive turns to the same side. For the humans this tendency is checked by the tendency to keep fairly close to the midline of the maze. This tendency is caused by the orientation of the different parts of the maze in relation to the human subject's body.

ZUSAMMENFASSUNG.

Vierzehn Ratten wurden geübt in dem Stone'schen T-förmigen Multipel-labyrinth den Weg zu finden, und zwanzig Studenten mit verbundenen Augen haben dasselbe Labyrinthmuster, allerdings als ein Griffel-Labyrinth eingeübt. Die Ratten haben das Labyrinth mit 5 Versuchen täglich eingeübt, und die Studenten in 2 Tagen mit einem Zwischenraum von einer Woche. Die Studenten brauchten im Durchschnitt 2/3 der Anzahl von Versuchen, die die Ratten brauchten om den Irrgarten zu lernen. Wegen der verschiedenen Methoden, die in den beiden Gruppen zur Verwendung kamen, sind keine endgültigen Folgerungen von diesen Experimenten herausgezogen worden. Wie bei ähnlichen Studien waren die Abweichungen grösser in den Versuchen mit Menschen als in den Versuchen mit Ratten Die „odd-even" Zuverlässigkeit (reliabitics) wurde erzielt mit Bezug auf Zeit für gepaarte und ungepaarte Versuche, und mit Bezug auf Fehlhandlungen für gepaarte und ungepaarte Versuche. Bei diesen Berechnungen hat man die beiden ersten Versuche teilweise berücksichtigt und teilweise ausser Betracht gelassen. Die Zuverlässigkeit ist im Allgemeinen am besten bei Versuchen mit Menschen. Mit Bezug auf Ratten hat es sich herausgestellt, dass die Zuverlässigkeit bei Correlation der Zeit bei gepaarten und ungepaarten Versuchen grösser ist, als die Correlation von Fehlhandlungen. Wie gewöhnlich bei Labyrinthversuchen fanden wir bei den Ratten weniger Fehlhandlungen im letzten Abschnitt des Labyrinthen als im ersten. Bei Menschen dagegen finden wir die grösste Zahl von Fehlhandlungen im ersten Abschnitt und die geringste Zahl in der Mitte. Die Verteilung von Fehlhandlungen innerhalb jedes Abschnittes ist unregelmässig, gewöhnlich finden wir in jedem Abschnitt einen Irrgang mit vielen Fehlhandlungen, während die anderen nur wenige Fehlhandlungen aufweisen. Dies betrachten wir als einen Beweis, dass andere Faktoren

als das Zielgradient tätig sind. Durch Untersuchungen der Fehlhandlungen in den verschiedenen Irrgängen, zeigt der Verfasser wie Menschen sowohl als Ratten geneigt sind, wechselweise Abbiegungen vorzuziehen, und Abbiegungen zu derselben Seite zu entgehen. Bei Menschen wird diese Neigung durch eine Tendenz, sich möglichst an der Mitte des Labyrinthen zu halten, abgeschwächt. Diese Tendenz stammt aus der Orientierung der verschiedenen Teile des Labyrinthen mit Bezug auf den Körper der Versuchsperson.

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A REPORT ON THE PSYCHOLOGICAL INSTITUTE AT HELSINKI UNIVERSITY

BY

MARTTI TAKALA

Psychological research in Finland had its first incentives from the Middle European seats of learning. The Gestalt psychology and its allied trends, i.e. dynamic and developmental psychology, have predominated up to quite recent times. At the outset the interest shown in the subject was marked by a theoretical approach, to which may be ascribed the fact that also applied psychology, which after the wars has gained ground rapidly, has been in close connection with theoretical research. Interest in psychology is on the increase; this year the preliminary course of Helsinki University was attended by nearly 250 students, of which about 10—20 per cent are majoring in psychology.

The facilities of research have not kept pace with this development. The "pioneer" of this field is Professor Eino Kaila, Member of the Academy of Finland, who founded the Psychological Institute of Helsinki University in 1932. Yet Psychology was recognized as an independent subject only in 1946, but the chair of Psychology became separated from that of Theoretical Philosophy as late as 1949. At present the Institute is headed by Acting Prof. Kai von Fieandt, who has two assistants, one of them for Applied Psychology.

In instruction constant emphasis is put on theoretical fundamental schooling and acquaintance with research and diagnostic methods. Practical training has up to now been perforce extra-curricular. In this respect new projects are in existence, though the realisation is under way. (As regards schooling, see Lehtovaara's report in Swedish, Nordisk Psykologi 1949).

In comparison with the corresponding institutes of greater countries, our facilities have been limited in many respects. Very

frequently it has been necessary to reduce the problems to conform with the practical, in the first instance technical conditions. All psychological fields have therefore not been subject to a research of theoretical significance, but attention has necessarily been focused on some special points. The first studies dealt mainly with perception, in which field Kaila's influence has been dominant. The other main objective may roughly be called psychology of personality, which branch also owes its start to Kaila. The special questions of heredity or Kretschmer's typology have been starting-points among this circle. The nature of research has changed later considerably as to this part, which has led to the special prominence of actual test psychology. The bulk of the publications have been issued in Finnish, mainly in "Ajatus", the year-book of Filosofinen Yhdistys (Finnish Philosophical Association).

Psychological research in Finland has not solely been concentrated on Helsinki University, but is carried out in other universities and also to a great extent in the practical institutes of the field (e.g. regarding clinical psychology, test investigation) that can offer better opportunities for working but remain in cooperation with the Psychological Institute.

Sensation and Perception

The first experiments in this field performed at the Institute concerned the phenomena of illumination. V. Fieandt analysed in his work the dependence of the constancy of illumination on learning and experience (Ein neues Invarianzphänomen der Farbenwahrnehmung, Ann. Acad. Sc. Fenn., 1938). Likewise he has dealt with the part that the direction of illumination plays in the perception of depth (Ueber Sehen von Tiefengebilden, 1938). Both inquiries are closely connected with the Gestalt psychology. For several years studies to ascertain the correlation between the various constancy phenomena of perception as well as the acoustic constancies and those occurring in the perception of direction have been prosecuted at the Institute.

The perception of movement has also been studied thoroughly. V. Fiandt's experiments concerning the judgement of the direction of movement in depth prove that coordinates (main dimensions) of perceptual space determine the Gestalt process. In the acoustic field the localization of moving source of sound

as well as the judgement of the moved distance have undergone close scrutiny. Further, there have been attempts (M. Takala) to analyse the determinants of the phenomenal speed of movement by varying both the figure and ground factors (partly in continuation of Brown's experiments, Pss. F. 10, 14). The mutual differences between various dimensions and directions have been worked out, and there has been an attempt to integrate them as a part into a wider summary concerning the asymmetries of the space.

There remains to be noted v. Wright's investigation dealing with the effect of the simultaneous contrasts in the tests made to prove the validity of Fechner's law.

Constitution and Heredity

Before the wars a great number of analyses and applications of Kretschmer's typology were conducted, e.g. by v. Fieandt, Helasvuo and Lehtovaara. These inquiries comprised the application of the typology to the Finnish population, the incidence of types in the range of attention, hand-writing, voice, criminality etc.

Lehtovaara has conducted large-scale investigations on heredity (Psychologische Zwillingsuntersuchungen, Ann. Acad. Sc. Fenn., 1938). Beside the customary comparisons between the identical and non-identical twins, he has paid special attention to their mutual social relationship and mimic reactions.

Tests and Applied Psychology

Practical reasons have caused the parallel development of these aspects. Among the investigators of applied psychology Oksala is to be noted for his industrial and vocational psychology studies. As he does not only bear in mind the direct practical application, his basis is wider, and he joins to dynamic psychology notions. Many examinations about the prospects and preferencies of youth as to job and vocation have been carried out, in which several interacting factors have been analysed. In the field of actual industrial psychology the limited facilities of the Institute have caused that recourse to specific institutes has been necessary.

On the other side, it has been possible to prosecute test investigations in ample measure, though in regular cooperation

with the practical establishments of aptitude testing. Vahervuo's doctoriate thesis consists of a scrutiny on mathematical aptitude (1948). Here use has been made of modern statistical methods of analysis. Later (1948) he cooperated with Puranen and M. Takala in a large-scale investigation which covers approximately 60 tests and aims, in its practical objective, at the development of an appropriate aptitude tests for students. Theoretically, it tends to analyse and compare intellectual, characterological, and interest factors in such form as is possible in the limit of group tests. Particular attention has been given to mathematical, verbal and speed tests. In addition, light has been thrown upon some methodical problems. As these tests have been designed to be rather difficult, they differ from the ordinary tests. The manipulation and exploitation of the material has been difficult in lack of technical equipment, but the results have been worked out in the main. The same investigators published earlier (1948) a study on the spatial factor.

Several other test studies have been performed or are being performed; e.g. Viitamäki's investigation concerning the verbal talent of school-age children and its dependence on characterological dimensions is under way. Recently an analysis on the factors of manual skill has been completed by A. Takala. Important, especially from the point of applied psychology is Lehti s inquiry into the selection of the air-force personnel.

Clinical Psychology

Clinical psychology has, partly owing to the impetus given by the wars, developed into a very extensive branch of study. This research is mainly conducted outside the Institute, though it, in the from of instruction and exercises, remains of great importance inter alia to students. Since 1941 a brain-injuries unit, in which neurosurgery, neuro-psychiatry, and psychology are brought to cooperate, has been in operation. The psychological field is represented with Mäki at the head. Attention has been focused increasingly on the psychopathology of speech: during the last few years 500 aphasia patients have undergone examinations.

Mäki has also carried out an extensive investigation about the pupils who have attended the municipal special schools for feeble-minded (morons) in Helsinki City during 1903—1939. In

the work he was concerned about the individual and social-pathological factors and proved that criminality and the need of social service exceeded what had been assumed.

Social Psychology

Kaila, and afterwards Koskenniemi, were to initiate social-psychological research in the country. Their studies were in the first instance sociometric: the correlation between friendship and social rang in different communities constituted the main problem. During the wars v. Fieandt carried on the treatment of these problems.

In close connection with the dynamic theory (Lewin, Brown) is Mielonen s investigation on panic, which is based on war-time material (1947). To the military circle belongs also Kretz's study on fighting morale and the concomitant factors, carried out in cooperation with the Physiological Institute, and still unpublished. The scope of the problem is wider than mere social psychology, the studies extend into somatic features and the field of differential psychology.

It is not possible to include in the above report certain studies on expressions, experiments as to the judgement of students and as to the estimation concerning the traits of other persons and halo effect connected with it as well as some other objects of study. The picture given here will be sufficient to illustrate to-day's principal problems. Many fields of psychology have so far been disregarded at the Institute, but this disadvantage will be removed by degrees simultaneously with the expansion of research and its emancipation from the post-war restrictions.

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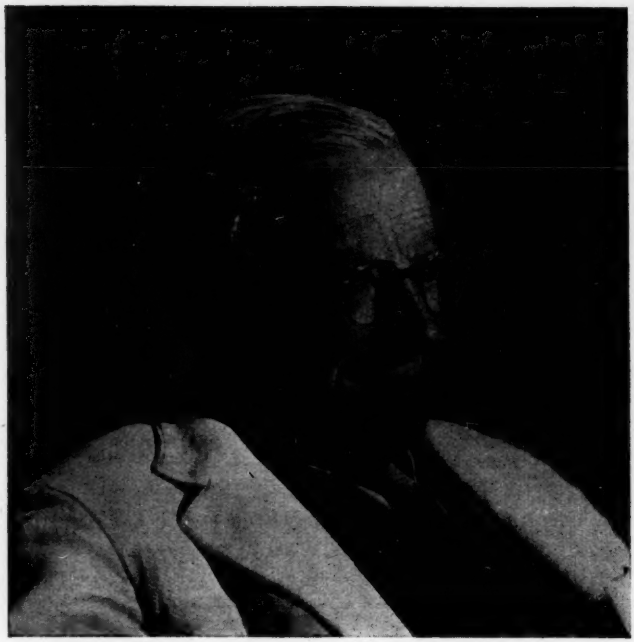

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SPECIAL ISSUE

IN HONOUR

OF

G. RÉVÉSZ

AT THE OCCASION OF HIS

70TH BIRTHDAY

WITH THE COLLABORATION

OF

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EDITED BY THE STAFF OF THE
PSYCHOLOGICAL LABORATORY OF THE
UNIVERSITY OF AMSTERDAM

GÉZA RÉVÉSZ

SCIENTIST AND TEACHER

A man reveals himself in many fields of social venture and in various sides thereof. The more eventful a man's existence both internally and externally, the more productive he proves to be in creating values, the more varied the aspects, under which his life can be considered and the heavier the task confronting his biographers. This task comprehends making a picture of a man's personality by drawing upon his life and work, a picture which should do justice to the multiplicity of the aspects as well as express the motives of his actions and activities, which disclose to us in the diversity of his vital manifestations the very essence of his being.

Notwithstanding his typically kind and helpful disposition, Révész is no easy subject for his biographers, even though they, like the present writers, may be familiar with his work and person by many years of co-operation. It is quite another thing to know Révész, even to know him well, than to give a picture of his colourful personality based upon his actions and scientific activities, a picture which should accentuate among the great diversity of traits the most characteristic ones in order to make us perceive this multiplicity and variety in a context of true significance. To illustrate some of these traits is the purpose of this article.

Gustav Kafka describes for us not only the personal friendships aspect, but also Révész' career. He shows us how life did not give Révész the repose and continuity in his work which a man desires, for whom science is the chief aim and interest. Often, especially as the years went by and time grew short he complained or rather felt vexed at losing so much valuable time owing to wars, revolutions and their consequences, time which he would have wished to make productive in concentration upon investigations and scientific reflection without having to give his energy to questions of social existence. Nevertheless we are very far off the mark as regards the reality of the motives, if we should try to explain the fact of Révész' productive activities

in so many and diverse fields of psychology by means of the external circumstances, which would have forced him to occupy himself with certain problems because of his "job". Of course if he had continued to occupy his chair in Budapest, he would not have given his attention to statistics regarding the results of college education in Holland, thereby greatly perturbing Dutch educationalists. But we are convinced that if he had remained in Hungary he would as surely have perturbed Hungarian educationalists by similar investigations, and this not from a desire of sowing unrest, from the sheer pleasure of upsetting people's applecarts, but from a desire, from the for him self-evident attitude to set well founded data versus vague statements based upon wishful thinking. With these data he wanted to lay a basis for a productive discussion and, if problems of practical life were concerned, for taking adaquate measures. Révész has no inclination whatever for floating discussions. Neither does he have any patience with statements, which flirt with problems — and do so intentionally, instead of getting a proper hold on them.

Conscientious investigations however could always count upon his full interest, even though the problems may have obtruded themselves by chance owing to external circumstances upon his notice. He occupied himself with these with the same intensity wherewith he attacks a self chosen problem. This state of being intrigued and completely absorbed by a problem, which crosses his way and which he finds worth while, is entirely typical of Révész. Herein his passion for investigating finds expression. It is especially roused and stimulated by seeking the method by means of which the problem in question can best be approached and solved. In his search for the adequate method, in his way of approaching a problem, the independence and originality of his mind reveal themselves, showing that he possesses the capacity of approaching a problem entirely unprejudiced. He feels neither bound to the manner wherein it was considered historically nor too much impressed by scientific problematics, prevailing at the moment. We believe that it is owing to these characteristic qualities of his mind that he was able to do so much pioneering in various and sundry fields of psychological research and that it were just those problems with a long and often confusing scientific history which drew his attention. He

possesses a mental independence, freshness, power and originality which enable him to approach and cultivate entirely without bias fields, heavily taxed with old and tainted problematics. In the beginning of his scientific career there were the problems of psychological acoustics, upon which he exercised his powers in many and original experiments. The most important results may be found in his "Zur Grundlegung der Tonpsychologie" (1913), results which have secured themselves a lasting place in musicology. Another large field of problems, to which he devoted many long years of investigation, is haptics. Here he posed afresh and developed the ancient problems of spatial perception, describing them in his "Die Formenwelt des Tastsinnes" (1938) (Psychology of the Blind 1950). At the same time already he grew engrossed in the problematics of the psychology of language. Here he was especially fascinated by the problem of the origin of language. These problems enabled him to give full scope to his passion for investigating methods of scientific thought, to enfold his gift of contemplating problems independently, his ingenuity in putting theories to the test. The product of this intensive mental effort, is the "Ursprung und Vorgeschichte der Sprache" (1947) (Origine et préhistoire du langage, 1950). This work is perhaps the most imposing manifestation of his scientific mind and creative powers. In one of the oldest and most confused fields of scientific history his penetrating brain brought clearness and order so that now at last a general survey is possible. On the one side he demonstrated most aptly the untenability of many of the former problem formulations, on the other side he clearly defined those aspects of the problem, which may be solved by the application of scientific methods, and those which are not suited to such process because of a lack of exact data. There are but few examples of a scientific work, where a so large and heterogeneous collection of facts, arguments and theories is arranged in so convincing and lucid a manner, and tested and judged as regards its consequences, so that in the end each component obtains its suitable place in the whole structure of arguments.

Finally we must still mention a special set of problems which occupied him during his entire scientific career, viz. the problem of talent. The "preliminary" results of his investigations will

be published shortly under the title "Talent und Genie".

If after these statements we look at Révész' valuable and many-sided works, we are able to discern a clear line therein. Notwithstanding the diversity of the problems handled by him a unity is perceptible in his way of approaching problems, in his independent mind and in his original total absence of bias.

Evidently these characteristic qualities of his scientific personality were bound to make Révész an individualist in science. One cannot characterize him according to a psychological school or trend. His trend is to be found in his own mode of scientifically approaching psychological problems in theory and practice.

He is no fluent orator ex cathedra, no teacher who can easily assimilate another man's thoughts, knowing how to reproduce them didactically correct and in a convincing manner. But wherever he worked — and it was in Holland that he worked longest — he attracted gifted young people, whom he stimulated by his passionate devotion to science, by his tireless labouring to improve and clarify the ultimate form of his mental efforts. Even as Révész because of his scientific constitution cannot be "nailed down" to any school, so he himself did not make a school. Neither did he wish to do so in the sense of a certain trend of research. He merely expects his pupils to work at the problems of their own choice and inclination with the same independence and originality of mind, with the same devotion to science and the same earnestness, which are so typical of himself. It is a good deal! May they succeed in fulfilling these expectations, thereby continuing in a world of increasing voluntary and forced collectivism the great European tradition of personal creativeness and responsibility, whereof Révész is so eminent a representative.

M. C. Bos

H. C. J. Duyker

GÉZA RÉVÉSZ

ERINNERUNGEN EINES JUGENDFREUNDES

VON

G. KAFKA (Würzburg)

In den Tagen, in denen ich diese Zeilen niederschrieb, werden es gerade 45 Jahre, dass ich zum erstenmal mit Révész zusammentraf, und eine so alte und in so vielen Stürmen erprobte Freundschaft darf wohl vom Recht der Geschwätzigkeit Gebrauch machen, wenn sie sich auf ihre Ursprünge zurückbesinnt. Ich erinnere mich noch wie heute an die Stelle auf dem Nikolausberger Weg in Göttingen, wo mir in den ersten Maitagen des Jahres 1904, als ich gerade von der Universität Wien an die Universität Göttingen übersiedelt war und mich in der fremden „preussischen" Umgebung noch recht unbehaglich fühlte, ein etwas schlaksiger Kommilitone entgegenkam und mich mit den Worten ansprach: „Du schaust ganz wie ein Oesterreicher aus; ich bin ein Ungar, und da gehört es sich doch eigentlich, dass wir hier ein bissel zusammenhalten, besonders da ich dich ja auch schon im Psychologischen Institut gesehen habe. Also erzähle einmal, wo du herkommst und was du hier willst!" Damit war eine Bekanntschaft geschlossen, die sich nicht nur während meines Göttinger Semesters zu einer echten Freundschaft vertiefte, sondern auch trübe und helle Zeiten, ja sogar zwei Weltkriege überdauerte! In Göttingen gab es wohl kaum einen Tag, an dem wir uns nicht entweder im Institut sahen oder Révész mich in meinen fürstlichen Appartements aufsuchte, — ich hatte das Glück gehabt, zwei kleine Kämmerchen zu ermieten, in deren einem ich sogar eine Pianino unterbringen konnte, — und es gehört ebenfalls zu meinen deutlichsten Erinnerungen, wie sich Révész auf diesem kümmerlichen Instrument zu ergehen liebte: besonders seine Wiedergabe der Mozartschen c-moll-Sonate, deren letzter Satz unter seinen Händen allerdings immer in einen Csárdás umzuschlagen drohte, wird mir immer unvergesslich bleiben. Nicht minder

sein gesegneter Appetit, wenn meine gute Mutter ihrem verwöhnten Söhnchen wieder einen grossen Karton häuslicher Bäckerei geschickt hatte, an der wir uns brüderlich delektierten, nachdem wir uns über den stilistisch wie inhaltlich gleich schwer lesbaren „Grundzügen" G. E. Müllers zur Genüge den Kopf zerbrochen hatten: — alte, unnennbare Tage!

Vielleicht hat diese Radotage für einen Dritten wenig Interesse, — aber ich habe meinen Erinnerungen deshalb die Zügel schiessen lassen, weil sich schon in ihnen die drei Charakterzüge offenbaren, welche die Persönlichkeit Révész' zeitlebens bestimmt und ihm auch später überall die Herzen gewonnen haben: die unablenkbare, ja geradezu verbissene Hingabe an eine Arbeit, die ihm seiner Anstrengungen würdig schien, die Gnade, sich verständnisvoll in jedes Kunstwerk versenken zu können, das ihm etwas zu sagen hatte, und nicht zuletzt die gesellige Bereitschaft, alle Annehmlichkeiten des Lebens in vollen Zügen zu geniessen!

Géza Révész is am 9. December 1878 zu Siófok am Plattensee in Ungarn geboren. Sein Vater war der Besitzer berühmter Weinberge und von ihm hat er wohl die feine Zunge geerbt für „die kleinen Dinge, die uns entzücken können". Seine Schulen absolvierte er in Budapest, legte seine Reifeprüfung am Obergymnasium daselbst im Jahr 1896 ab und bezog dann die Universität Budapest, um Rechtswissenschaft zu studieren. Er blieb bis zum Jahr 1902 an der Universität Budapest immatrikuliert und unterbrach seine Studien nur, um im Jahr 1898/9 sein Freiwilligenjahr beim k.u.k. 10 reitenden Artillerie-Regiment abzuleisten; zum Reserveleutnant wurde er im k.u. Husarenregiment Nr. 1 ernannt. Aber sein Interesse für Rechtsgeschichte und vergleichende Rechtswissenschaft trat im gleichen Masse zurück, wie sich sein Interesse an psychologischen Problemen vordrängte, und so reiste er noch am Tage seiner Promotion zum Dr iur. von Budapest nach Göttingen ab, um sich dort ganz dem Studium der Psychologie unter Leitung von G. E. Müller zu widmen. Seinen Aufenthalt in Göttingen unterbrach er für ein Jahr, um in Berlin in den Instituten von Karl Stumpf, Engelmann und Dubois—Reymond zu arbeiten, fand sich aber wieder in Göttingen ein, um bei Müller im Jahre 1905 mit einer Dissertation „Ueber die Abhängigkeit

der Farbenschwellen von der achromatischen Erregung" zum Dr. phil. zu promovieren.

Nach Budapest kehrte er im Jahre 1906 zurück, wurde zunächst Assistent bei dem Physiologen Tangl und richtete in dessen Institut ein kleines psychologisches Laboratorium ein, das erste seiner Art in Ungarn. 1908 habilitierte er sich für experimentelle Psychologie und erhielt alsbald von der Stadt Budapest den Auftrag, ein Laboratorium für psychologische Pädagogik einzurichten und zu leiten. (Diese Laboratorium besteht auch heute noch und bildet den Mittelpunkt der psychologischen Forschung in Ugarn.) Gleichzeitig wurde er zum Dozenten für Psychologie und Soziologie an der k.ungarischen Kriegsakademie ernannt.

Im Jahre 1910 vermählte er sich mit Magda Alexander, der Tochter Bernhard Alexanders, der an der Budapester Universität das Fach der Philosophie, an der Technischen Hochschule das Fach der Aesthetik vertrat. Magda, die 1909 in Kunstgeschichte promovierte, gehörte mit ihrem Bruder Franz, dem heute so berühmten Psychiater der Universität Chicago, und mit dem verstorbenen Soziologen Karl Mannheim, später Prof. in London, zu Gézas ersten Hörern. In den Jahren 1910—14 entfaltete Révész eine reiche akademische und publizistische Tätigkeit. Als Frucht seiner akustischen Studien erschien 1912 seine „Grundlegung der Tonpsychologie", die besonders Franz Brentanos Interesse erregte.

Im ersten Weltkrieg rückte er 1914 ein und nahm am serbischen und russischen Feldzug teil. Er wurde erst zum Oberleutnant, dann zum Rittmeister befördert und erhielt verschiedene militärische Auszeichnungen. 1917 wurde er mit der Einrichtung und Leitung der Eignungsprüfungen in der k.u. Armee betraut. Im gleichen Jahre wurde er an der Universität Budapest zum a.o. Professor für Psychologie und zum Direktor des von ihm begründeten Institutes, 1918 zum Ordinarius ernannt. Nach der Machtergreifung durch Horthy im Jahr 1919 entschloss sich Révész, Ungarn zu verlassen und freiwillig auf sein Ordinariat zu verzichten. Nach kurzem Aufenthalt in München und Heidelberg erhielt er von Heymans (Groningen) und Zwaardemaker (Utrecht) eine Aufforderung, Vorträge in Holland zu halten, die so grossen Anklang fanden, dass ihm zunächst im Rahmen einer pädagogischen Institution ein kleines,

später ein grösseres Laboratorium in Amsterdam eingeräumt wurde. Zugleich wurde er in der wirtschaftswissenschaftlichen Fakultät der dortigen Universität mit der Abhaltung von Vorlesungen über Betriebspsychologie beauftragt und von mehreren grösseren Firmen als psychotechnischer Berater verpflichtet. In dieser Zeit veröffentlichte er seine „Einleitung in die Betriebspsychologie".

1931 wurde an der Universität Amsterdam ein Lehrstuhl für Psychologie geschaffen, auf den Révész 1932 als Extraordinarius berufen wurde. Seine Ernennung zum Ordinarius erfolgte einige Jahre später. Sein jetziges Institut Keizersgracht 613 wurde 1939 feierlich eingeweiht. Es bestand ursprünglich aus einem alten zu Laboratoriumszwecken umgebauten Patrizierhaus, erwies sich aber bald als unzureichend und wurde daher durch Einbeziehung des Nebenhauses erweitert, so dass dem Institut heute 40 Räume und ein Hörsaal zur Verfügung stehen, in denen Lehre und Forschung mit gleicher Intensität betrieben werden. Das Institut dürfte damit das grösste aller psychologischen Institute Europas sein.

Die Liste der von Révész veröffentlichten Arbeiten umfasste schon in Jahre 1948 nach einer Zusammenstellung des Bibliographical Research 119 Titel. Sie erstrecken sich auf die Gebiete der allgemeinen Psychologie, der Sinnes- und Wahrnehmungspsychologie, der medizinischen, sozialen und pädagogischen Psychologie, der Sprach-, Begabungs-, Kinder- und Tierpsychologie.

Révész ist Vorstand und Ehrenmitglied zahlreicher wissenschaftlicher Verenigungen, Mitglied der bayerischen (1949) und der ungarischen Akademie der Wissenschaften (1946), Ehrendokter der Universität Wurzburg (1949), Begründer und Hauptschriftleiter der Acta Psychologica (seit 1935), Hauptschriftleiter der Nederl. Tijdschrift voor Psychologie, Mitherausgeber der Algem. Nederl. Tijdschr. voor Wijsbegeerte, Begründer des Nederl. Instituut van Practizerende Psychologen. Seine Aemter und seine Leistungen kennzeichnen ihn in gleicher Weise als eine der markantesten Persönlichkeiten in der Psychologie der Gegenwart. Das Lebensbild Révész' wäre aber unvollständig, wenn man nicht seiner Frau gedenken wollte, die nun schon bald 40 Jahre Freud und Leid mit ihm geteilt hat. Gewiss hat ihr Einfluss wesentlich dazu beigetragen, dass sich Révész verhältnismäs-

sig bald von der ausschliesslichen Einstellung auf sinnespsychologische Probleme befreit und seinen Untersuchungen viel weitere Ziele gesteckt hat, als es in der wissenschaftlich durchaus soliden, aber doch immerhin etwas phantasielosen Atmosphäre des Müllerschen Institutes möglich gewesen wäre. Zahlreiche gemeinsame Reisen und viele Gespräche mit ihr über Kunstfragen haben ihm die Anregung zu manchen psychologisch-aesthetischen Themen gegeben, die dann in seinen musik- und begabungspsychologischen Arbeiten, aber auch in seiner Blindenpsychologie ihren Niederschlag gefunden haben.

Andererseits ist der psychologische Einfluss deutlich in den Arbeiten von Magda Révész erkennbar, schon in ihren ersten Büchern: „Die alten Lagerhäuser Amsterdams" (1927) und „Byzantijnsche Kunst in Italië" (1938), noch stärker in ihren Arbeiten, in denen sie ausschliesslich kunstpsychologische Probleme bearbeitet, wie z.B. „Der Turm, Versuch einer kunstpsychologischen Interpretation", und „Eva, Maria, Venus als Symbole des weiblichen Prinzips in der bildenden Kunst", die in Kurzem erscheinen werden.

Mag sein, dass ich in diesen Zeilen am meisten von meinen persönlichen Beziehungen geredet habe. Aber für den alten Freund ist es doch der menschliche Gehalt, der den Ausschlag gibt, zumal wenn sich die wissenschaftlichen Fähigkeiten des Jubilars so eindeutig bewährt haben, dass es Eulen nach Athen tragen hiesse, wenn man sie noch mit viel schönen Reden zu preisen gedächte. Wie hoch die Mitwelt ihren verdienten Zeitgenossen schätzt, hat sie ihm oft und ehrenvoll genug bewiesen. Wir Wenigen aber, die von seinen alten Freunden noch übrig sind, wünschen ihm, dass er uns in voller geistigen und körperlichen Frische überleben und, uns allen voran, auch weiterhin zur Förderung unserer gemeinsamen Wissenschaft beitragen möge!

THE EVOLUTION AND PRESENT TRENDS OF PSYCHOANALYSIS [1])

BY

FRANZ ALEXANDER (Chicago, Ill.)

Psychoanalytic therapy from the beginning has been founded upon theoretical concepts concerning the underlying causes of mental disturbances. The development of psychoanalytic therapy therefore can be understood in the light of advancement in the knowledge of the human personality. In spite of changes in theory, the fundamental conception that psychoneurotic and psychotic symptoms are the manifestation of psychological tendencies which the ego could not harmoniously incorporate in its unity has remained the foundation of psychoanalysis. Accordingly, the aim of therapy crystallized itself in the task of establishing the causes of the ego's inability to harmonize and utilize in a constructive manner some impulses and, after having established them, to eliminate these causes.

In the early phase of psychoanalysis, the main interest of Freud and his followers was to identify unconscious mental content and to understand its archaic language. This is the art of interpretation which consists in reconstructing from conscious material the hidden unconscious factors.

More recently the interest has shifted to the study of the ego's functions and its failures since mental disturbances are considered as failures of the ego functions. We are assuming four basic ego functions: 1) internal perception of instinctive needs; 2) external perception of existing conditions upon which the gratification of subjective needs depends; 3) the integrative faculty by which the ego coordinates instinctive urges with each other and with the requirements of the superego and adapts them to the environmental conditions; 4) the executive faculty

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[1]) This article is based on a report given at the Congres International de Psychiatrie, Paris, September, 1950, under the same title.

by which it controls voluntary behavior. Through the latter the ego can implement the results of its integrative function which consist fundamentally in the rational cognitive faculty. Only gradually does the ego develop the capacity to coordinate psychological impulses with each other and to adapt them to the external conditions in a way that assures the best possible deal in a given situation. To accomplish this, the ego must learn to postpone certain desires when their satisfaction might endanger more important ones. It has to learn to compromise, to modify the desires, to subordinate less important to more important needs. In other words, it learns what is considered rational behavior.

For psychopathology, it is important to realize that this coordinated rational behavior can be maintained only by a constant struggle on the part of the ego, because the instinctual tendencies retain their original inclination for immediate gratification. This is the basis of Freud's structural concept in differentiating an ego from an id. He assumed that impulses retain their original tendency for noncoordinated isolated gratifications. This tendency manifests itself in all those psychological phenomena which are not rational or coordinated, such as dreams, free fantasy, impulsive behavior and all that is known as psychopathological. Whenever the ego is threatened by impulses which are not in harmony with its accepted standards or reality, a conflict and concomitant anxiety arises. To anxiety the ego reacts by defenses which are directed against these internally threatening tendencies. The most important defense is repression. Other mechanisms are either defenses which favor repression, such as over-compensation or rationalization or vents by which the repudiated tendencies can find an outlet such as projection, substitution, displacement or turning impulses directed against external objects against the self. Mental disease represents a failure of the ego to secure gratifications for subjective needs in a harmonious and reality-adjusted manner and a breakdown of the defenses by which it tries to neutralize impulses which it cannot harmonize with its internal standards and external reality.

As has been said before, in its main phases, psychoanalytic therapy has followed the development of theory. When the dynamic influence of unconscious tendencies was discovered,

the therapy consisted in bringing repressed psychological content into consciousness. Cathartic hypnosis was such a procedure, a device by which the ego's rejection of repressed material was circumvented by the artificial hypnotic state. Freud soon realized that the mobilization of repressed material is not sufficient to cure neurotic symptoms but that the ego must undergo changes in order to become capable of integrating repressed material in its system. The therapeutic aim consequently must consist in achieving the required changes in the ego. In spite of this new orientation, the center of interest shifted only slowly from the understanding of unconscious material, the knowledge of symbolism, the art of translating the archaic picture language of the unconscious into verbal thinking — to the study of the defenses of the ego. The crucial discovery concerning the ways and means by which the ego's defenses can be influenced was that of the transference phenomenon. Its therapeutic significance was only gradually recognized.

The importance of the patient's emotional experiences in relationship to the analyst became more and more appreciated and, with this emphasis, the handling of the transference and the patient's resistance became the axis of the therapy. The present concept of psychoanalytic treatment can be formulated as follows: The neurotic condition is the result of the ego's failure to accomplish its integrative function of mediation between conflicting or partially conflicting needs and desires and their adaptation to environmental conditions. This faculty to a high degree depends upon the history of a person's emotional development. The most significant factor interfering with adaptive behavior which has always been recognized by psychoanalysts is related to the basic mechanism of repression. Repression is the characteristic defense measure of the weak ego of the child who cannot control successfully those desires which appear in his consciousness and which are in conflict with the requirements of the environment or with other subjective needs. He has only one way to save himself from the painful experiences he was subjected to in the past when he gave in to such impulses: he has to exclude them radically from his consciousness. This saves him from a conflict with reality and/or internal conflict, but at the same time creates a frustration.

Eventually the repressed impulse will seek outlet in symptoms. The ego is deprived of the repressed dynamic force which it could utilize if it were able to integrate the force within its system.

Recently French subjected the integrative function to a careful study in which he tried to evaluate not only the chronic factors which have an influence upon it, but also its temporary quantative variations. Excessive intensity of an emotional need, for example, makes effective integration more difficult because urgent needs tend to seek immediate gratification and thus interfere with the often required postponement of immediate satisfaction. Low intensity of motivations may also decrease the integrative faculty and anxiety, too, depending on its intensity, may paralyze or favor the integrative function. Past successes and resulting hope increase integrative ability; consistent failures impair it.

The highest form of integrative function requires conscious deliberation. Everything which is excluded from consciousness is beyond the reach of the ego's highest integrative functions. Neurotic symptoms are like foreign bodies and represent isolated substitute gratifications which are the source of conflict and suffering. Psychoanalytic therapy aims at the extension of the ego's integrative scope over repressed tendencies by making them conscious. For this purpose it attempts to mobilize unconscious material. In order to overcome repressions by systematic psychological maneuvers one must know the causes of repression. The child represses those tendencies the expression of which caused him painful experiences, such as physical suffering, punishment, withdrawal of parental love and resulting insecurity. The emergence of such a tendency constitutes a danger to which the ego reacts with anxiety. Essentially this process is similar to conditioning. The sequence of events has three links, 1) the emergence of the impulse, 2) acting upon it, and 3) the painful results. Originally the anxiety was aroused by the memory of a painful experience. After repetition the anxiety becomes associated with the first link, with the emergence of the impulse itself which becomes repressed and thus excluded from motor expression; in this manner the resulting pain is avoided.

Psychoanalytic therapy in this light reveals itself as a process

of reconditioning. The ego is induced to face a repressed impulse by eliminating the anxiety which induced repression. This is achieved by reproducing the original situation but changing the conditions so that they lose their anxiety-producing effect. As soon as the patient senses that the analyst's response to expression of his impulses is different from that of the parents, their intimidating effect is removed. The aim of the therapy consists first in reviving the interpersonal situation which led to the original repressions and second, supplying a new kind of experience which is suitable to undo the effects of the parental responses. Accordingly, the analyst's response to the patient's material should be the opposite of the parental reactions. This can be achieved only if the analyst is able to reconstruct the pathogenic parental influences and respond to the patient's emotional manifestations in a manner appropriate to counteract and neutralize the disturbing influence of the parents. Essentially this is nothing but emotional reconditioning.

The objective non-evaluative attitude of the therapist which Freud recommended undoubtedly is different from anything the patient experienced before because objectivity without any emotional involvement does not exist in ordinary human relationships. The corrective influence of this objective attitude can be further enhanced if the therapist's reactions are specifically calculated to counteract the effect of parental reactions.

The practical conclusion from all this is that in place of his spontaneous countertransference reactions the therapist must assume an attitude towards the patient which in the light of the patient's history appears appropriate to undo the pathogenic influences of the parents. In this way the emotional experiences in the therapy will have a corrective influence resulting in the lifting of repressions. The patient will be able to face what he formerly repressed because of parental censure.

All this does not disprove, however, the value of insight. By his interpretative work the analyst assists the patient's ego to integrate the new material liberated from repression. Making conscious, however, what was hitherto repressed requires the reduction of anxiety. This is achieved not only by the corrective emotional experience but also by insight which in itself has an anxiety-reducing effect. The ego's function is mastery through insight. The integrative function is based on the appraisal of

the total situation — both internal and external. Hence interpretative work increases the ego's self-confidence in dealing with newly uncovered material. Something a person understands loses its threatening quality; understanding means mastery. Properly devised attitudes and correct interpretative work together constitute psychoanalytic therapy.

Another important development is the special attention given to the patient's dependent cravings which always tend to prolong the treatment.

By its very nature the analytic technique necessitates the establishment of a dependent relationship between patient and therapist in order to allow the patient to relive and face again the old unresolved interpersonal relations with the parents. Inasmuch as it encourages dependence, the analytic technique which is divised to cure the neurosis carries in itself a factor which prolongs the neurotic condition. In order to resolve infantile reactions, one feature of which is always dependence, one must reproduce them in the transference; only then can one combat them. Much of the recent technical experimentation concerns this inherent difficulty of the analytical technique. Prolonged uninterrupted daily interviews in many cases favor the development of an intensive interminable dependent relationship and consequently postpone recovery. To overcome this weak spot of the analytic technique is one of the crucial technical issues. From the therapeutic studies of the Chicago Institute for Psychoanalysis, a series of technical recommendations resulted. The essence of them is that from the beginning, the therapist must be aware of the danger inherent in the regressive tendencies of the patients. To counteract this danger the analyst must consistently force upon the patient as much independence as possible. Interpretations alone cannot accomplish this. The dependent tendencies must be counteracted by reducing the contact with the patient to that minimum which is necessary to preserve the continuity of the treatment. Properly timed reduction of the frequency of interviews, shorter and longer interruptions are indispensable in every case. Encouraging the patient to new life experiences outside of the treatment suitable to increase self-confidence and encourage hope are also potent devices in weaning the patient from the dependent relation to his therapist.

Most important in these recent developments is the awakening of a fresh, undogmatic experimental spirit, the abandoning of a trend which was inclined to consider psychoanalytic theory and therapy as a static system which should be conserved in the form in which Freud left it to his followers. This new experimental attitude is truer to the heritage from Freud than the static one. Freud himself was one of the great experimentalists of all time who, in his relentless search for truth, was never averse to changing his previous concepts and therapeutic procedures whenever advanced knowledge required.

HUMAN TOLERANCE LIMITS

by

Sir FREDERIC BARTLETT, C.B.E., F.R.S., (Cambridge, England)

It was with great pleasure that I received and accepted an invitation to contribute to this volume in honour of Dr Géza Révész. The pleasure is tempered by the knowledge that the occasion marks the official close of a long and distinguished career in he service of psychological science. But an official close is perhaps rarely of much significance beyond the purely personal, and all psychologists, wherever they may be, who have had the good fortune to meet and converse with Professor Révész, to correspond with him, or to study his own contributions to research, will hope that retirement will bring him not only many years of greater ease, but also enhanced freedom to continue his striking and original contributions to the advancement of human knowledge.

The topic which I have chosen to consider in the present paper is one which has both practical and fundamental significance. In a world of growing difficulty people everywhere tend to be pushed farther and farther towards the limits of normal human endurance. In many directions it has already become a matter of profound importance that we should be able to assess and measure these limits in terms of their internal and external conditions. But when we set out to do this we come across basic problems concerning the nature of neurophysiological and psychophysiological organisation, the adequate treatment of which must have a bearing, as yet very little recognised, upon the whole methodology of the controlled study of human performance.

The material of this paper was first delivered, in the autumn of 1948, at the National Hospital for the Study of Nervous Diseases, Queen Spuare, London. The greater part of the work upon which it is based has been carried out at the Laboratory for Experimental Psychology, Cambridge during very recent years. Most of this work is due to the experimental insight and

skill of Dr N. H. Mackworth, a member of the Medical Research Council Cambridge Unit for Research in Applied Psychology.

It is hardly too much to say that every fruitful psychological investigation begins from some perfectly simple and obvious observation about human behaviour which anybody can make and usually many people do make. That is certainly the case with the investigations which I am going to discuss in the present lecture. The senses, and all the other equipment of human reaction, have undergone an immensely long process of development, but all the time in the closest relation to an environment which is in constant, and often in very rapid change. If every measurable environmental change had an associated significant change in human experience or conduct, life would be intolerably spasmodic, episodic and lacking in consistency and stability.

In a way, the whole development of experimental psychology is founded upon the demonstration, by Weber, that not every measurable change of amount or intensity of stimulus gives rise to an appreciable difference of sensory response, and also that whenever a sensory response appears to be just more, or just less than another of the same kind occurring immediately before, this difference, within fairly wide limits, is correlated with a constant proportional increase or decrease of the amount or intensity of stimuli of the appropriate mode. Yet these demonstrations did almost as much harm, by obscuring a number of tremendously important problems of behaviour, as they did good by introducing methods of precision into a field which up to that time had been intractable to any quantitative treatment. They have led to an immense amount of laborious and detailed experiment, usually relegated to the psychologist, devoted to obtaining exact measures of minimal discriminable differences and series of differences in the case of each sensory mode. These have told us a large amount of valuable things about how the single sense responds to single dimensional changes of isolated types of stimulation. They have led also to much clever, interesting and interminable speculation about the possible relation of these minimal discriminable changes at threshold to behaviour involving perception and other complex functions.

There is no more a definite answer to this, or a promise of a definite answer, than there is to the comparable, though very different, question of how observable forms of human group behaviour are precisely related to the multitudes of individual reactions on which they seem to be built.

Let us look at a case or two with our first observation in mind, that the mechanisms of human response have grown up in a world of change and have produced behaviour which is, nevertheless, relatively stable.

Anybody with normal vision will be able to read a page of printed material in ordinary type at an illumination of about 1 foot candles. Now increase the illumination. There is certainly no improvement of reading efficiency with every discriminable rise. There is an improvement at intervals much bigger than the discriminable intervals up to about 2—3 foot candles, and thereafter no noticeable change until the level of illumination is so high that the whole reading task, for other reasons, breaks up. This is not to say, of course, that there are no changes at all, but that the changes are not a matter of sheer reading efficiency itself, but of other conditions. And even so, none of them follows directly the series of just noticeable changes in environmental stimuli.

Or again, set up a screen containing letters, or words, at right-angles to the line of vision, and provide the very best possible conditions for legibility. Tilt the screen gradually backwards so that the upper part of it is farther from the eyes of the observer than the lower part. He will notice the tilt, and again and again the increase of tilt, long enough before he shows any significant change in the delicacy or efficiency of his appreciation of the material. But a very small increase of tilt beyond a third of a right-angle will upset suddenly and markedly the accuracy of his reading.

There are innumerable illustrations of this sort of relation between response and its conditions in all kinds of behaviour, sensory, perceptual, motor, or, involving the operation of the higher mental processes.

They introduce us to the problem of the human ceiling, or of human tolerance limits. These are of vital interest and importance alike for theory and for practice. They call for controlled investigation whether we are set to study the deter-

mination and range of normal behaviour, or whether our aim is to explain the causation of abnormal conduct and perhaps to find a way to avoid or remedy it. I shall first briefly describe the main results in a few instances which have been made the subject of detailed investigations.

Towards the end of the war it became likely that very large numbers of men would be required to carry out Service operations under conditions of very great heat and humidity. It was resolved therefore to design and carry out experiments which might show how the behaviour of normal, fit individuals is likely to be affected by these environmental conditions. A large and important part of the work was done elsewhere, but I am concerned only with the direct studies of human performance which we carried out at Cambridge. We decided that we must choose several appropriate kinds of continued exercise, keep them going for the length of normal working spells, and have a running record of the level of performance and variations of the level of performance throughout. We planned experiments to include work involving heavy physical effort, work requiring moderate physical effort, work demanding light physical effort and moderate mental skille and work calling for hardly any physical effort at all, but mental skill of a high level. All operators were first thoroughly acclimatised. The results showed consistently and conclusively very little change in the level of proficiency of performance until a critical region of heat and humidity increase was reached and then a very rapid and decisive breakdown. For all types of operation the critical region lay between the effective temperature readings of 83° F. and 87.5° F. (that is, of Dry to Wet Bulb readings of 90°/80° F. and 95°/85° F.) when the air movement is 100 ft. a minute. When this region was reached efficiency of performance began to be seriously affected, and a small change above these levels was capable of producing a much more important variation of performance than a large change below them.

Comparatively few people living in England are likely to suffer much from atmospheric heat, but there are two other forms of environmental stress which are of increasing importance in this and every other country in the world that is usually considered "up to date". These are "load stress" and "speed stress", when load means the number of effective items

which have to be simultaneously, or near simultaneously, attended to, and speed is defined in terms of the average amount of time allowed for dealing with any item, or the overall time allowed for a complete group or cluster of items.

The burdens of load and speed usually increase together and both of them are tied up with the expanding use of machines. Particularly when electrical and electronic designs replace earlier mechanical ones in any technical or industrial field it is a very common result that the operators both have more to do and have to do it more quickly. That speed and load stress already present problems that are particularly acute in every type of industrial production with a limited field of recruitment is admitted everywhere.

We have therefore begun some investigations of the effects to be expected from increase, both separately and conjointly, of load and speel stress. The principal practical situation that we had in mind was a radar controlled traffic director system, or similar operation of that type, in which a number of decisions have to be made in a limited time, the data for the decisions being simultaneously presented by collection from a number of sources. Experiments can be arranged in which the rate at which decisions must be made, the number of decisions to be made at a given rate, and the amount of information simultaneously displayed on the basis of which the decisions are made, can all be controlled. Undoubtedly the general problems of load and speed stress must be studied in many other types of situation as well, but so far there is complete demonstration that the principle of ceiling or tolerance limits applies just as well here as it does in the case of atmospheric stress. In our experiments, for example, the time allowed for a decision could be reduced from 5 to just short of 2 seconds without any significant increase of mistaken judgment, but with any small reduction after this point the errors shot up. Similarly, but in our case less dramatically for load. As the number of items simultaneously under consideration rose from 1 to between 10 and 15 there was no marked increase in the frequency of errors of judgment. But any increase in this neighbourhood and above produced a significant rise of error. Particularly when speed and load both increase the rise of error at the critical load range becomes very strongly marked indeed. It seems quite certain that if work is

already being done at a high speed and the load is now increased, it is a hopelessly wrong method to calculate the number of items increase and allow a time extension over the whole task in the same proportion. This is apparently commonly done, but it is a complete mistake and will impose a danger of all kinds of errors and misunderstandings and probably of ill-health if the load is within the critical range for whatever work is involved.

Like nearly everything else in human behaviour tolerance limits have no completely sharp cut-off, the same for all. There is a bounding range, and the marked deterioration which is a sign that individual tolerance has been surpassed may occur at any position within it. This raises some exceedingly interesting and important problems concerning what general conditions can apparently extend human tolerance limits to full stretch without abnormal strain. Most of the work on this is still to be done, but a few interesting things have been established.

There is demonstrative evidence that a high level of skill exhibited under conditions well within the tolerance limit can push the limit back, but not by very much. The reasons are not yet well established, but it is probable that in the case of skill requiring considerable muscular effort the most important factor is economy of actual bodily movement; and in the case of mental skill, reduction in the number and detail of cues that are necessary, and a consequent relative escape from distracting conditions of work.

More interesting, both from a theoretical point of view and for its immediate topical importance, is the influence of incentive. There are two forms of this which have proved amenable to experimental control. First there is the incentive which offers a special reward for high level of performance, and secondly there is the incentive which operates as an integral part of the operation itself, and is not merely a sequel to a job well done.

The first form was investigated in the case of high atmospheric temperatures and humidity. Targets, high but attainable, were set and a much valued reward given when they were reached or surpassed. Under the experimental conditions the immediate effects of these incentive conditions were an overall rise of amount of work of nearly 60 %. The best operators, that is those who were both most skilled and most conscientious,

improved more than twice as much as the others, but all of them, good, average and poor alike fell off in the same relative amounts when the tolerance limit was reached. Hence the absolute deterioration of the better operator was greater than that of the less good operator. These results can be applied to short term effort only. There are obviously severe limits to the use of this type of incentive.

Study of the second form of incentive, one which is built into the task performance itself, opens up an immense new field of experiment. For some time it has been known that some kinds of what is called "knowledge of results" may have a tremendous effect in the learning and maintenance of any suitably complex human function.

In 1947 Leonard Carmichael and W. F. Dearborn published a very extensive and thoroughly controlled comparative and experimental study of *Reading and Visual Fatigue* [1]). Under the best possible conditions of legibility they required their observers to read ordinary print, or microfilm, for long periods of continuous exercise. At first, with two-hour spells, they obtained plenty of evidence, mounting as the work proceeded, of reading errors, of eye-movement regressions, and of a wandering scan in which the eyes moved but nothing at all was being discriminated. Then they introduced, every twenty minutes, what could be called a "confirming reaction". That is the reader had to go on working all the time, but he obtained information which he could use to check up whether he was maintaining his initial standard or not. It was, in fact, a regularly intermittent use of the technique of "knowledge of results". All the original signs of deterioration now disappeared. Reading proceeded up to six-hour spells with no significant increase of errors or of regressive eye movements, and with no wandering scan. There were also no detectable delayed after effects. At first blush this result seems almost incredible. But as soon as I read of it I was reminded of the achievements which I have several times witnessed of expert air pilots in the Cambridge cock-pit in a task also requiring constant alertness of interpretation of exteroceptive signals, but little physical effort. Given a chance to check up his performance every fifteen

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[1]) New York: Houghton Mifflin Company, pp. XIV + 483.

minutes the keen and good pilot would keep going with no significant fall of speed or accuracy for very long periods. In this case also, as in the reading experiment, no detectable delayed, or after-effects followed.

At the present stage interpretations are hazardous and must be taken simply as an invitation to further experiment. But if that is understood I think suggestions can reasonably be made.

There may be an essential difference between incentives that are tacked on to a task, all systems of reward and punishment, for example, and incentives that operate within the prosecution of the task itself. The former can raise the amount and efficiency of performance for short periods but can do nothing at all to push up the ceiling of tolerance, when this is imposed by objective environmental conditions. The latter can raise the level of efficiency in the learning stage and can maintain this level when mastery is reached. In one respect at least it can push up the ceiling of tolerance, for it can prolong the effective working spell without imposing any increase of strain. Whether it can cope with other objective conditions of human tolerance nobody yet knows.

If certain forms of "knowledge of results" can apparently have effects of the kind I have just mentioned, what is likely to happen in any kind of continued exercise if sufficient forewarning of impending stimuli is given to facilitate anticipatory responses? It has been a matter of great surprise to some that whereas psychological experimenters and others have made most elaborate studies of remembering, very little well designed or controlled investigations of the range and variation of anticipation have been attempted. Evidence is, however, beginning to accumulate that forewarning, rightly spaced, may very likely be found to push up the ceiling of speed tolerance. For load stress the evidence as regards effect on tolerance limits is at present entirely inconclusive. If the effect on speed stress can be confirmed, it is extremely probable that the mechanism involved is that forewarning increases the effective range of anticipation and so makes it possible for the operator to work efficiently with less or reduced cues.

As I see it, the fundamental theoretic interest of these two internal incentive conditions is that they are essentially an appeal to *understanding*. They operate through cognition and

so have a kind of long-range stabilising effect, different from the short term temporary effect of the external incentive whose appeal is largely emotional. Both of them seem most appropriately considered as a form of confirming reaction, or to use an electrical analogy, of feed-back, the one enabling what is in fact achieved to be compared with what it is desired to achieve, and the other making it possible to prepare for what is going to be done without prejudice to the success of the immediate undertaking.

If what I have said is well founded, and as far as it goes this is certainly the case, it may give us a much more controlled approach to the whole study of normality. For normality may then be defined, so far as human behaviour goes, not merely descriptively as a collection of reactions lying within very wide limits of minor differences, but as the capability to withstand assigned stresses up to certain critical borders which can all be experimentally ascertained.

It will, of course, be fully understood that everything I have said refers only to day-to-day working conditions, and has no bearing whatever upon those prodigious efforts of endurance of which men and groups are capable in the face of sharp challenge to their deepest instincts or to their most profound beliefs.

QUELQUES REFLEXIONS EPISTEMOLOGIQUES SUR LE PARALLELISME

PAR

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En psychologie on entend ordinairement par le terme de parallélisme qu'à tout phénomène psychique correspond un processus cérébral déterminé et un seul, tel qu'à chaque changement du premier réponde un changement du second. Ce processus cérébral peut être d'une grande complexité diffuse, et l'adjectif «déterminé» a plutôt le sens de «déterminable». Mais en tout cas, d'après cette théorie on est en droit de croire que tout phénomène psychique s'accompagne d'un phénomène cérébral. Pour désigner cette théorie on emploie quelquefois le terme de «parallélisme empirique», parceque des observations et des recherches empiriques ont donné naissance à cette théorie. En outre, par l'adjectif «empirique» on peut souligner la distinction qu'il faut faire entre cette théorie elle-même et des explications métaphysiques auxquelles cette théorie a mené.

Nous savons qu'une grande quantité de physiologistes ont interprété la théorie en ce sens qu'ils ont considéré la vie psychique comme un épiphénomène du cerveau et que cette conception s'est formée en eux si spontanément qu'ils ne se sont pas même rendu compte qu'il y avait là une interprétation. La théorie du parallélisme nous place devant un problème, elle exige une explication, mais il y a différentes explications. Par ce fait déjà il est facile de discerner l'explication et ce qui est le point de départ de l'explication. Tout de même, pour beaucoup de gens, la confusion est tenace et ainsi il y a raison de distinguer expressément le parallélisme empirique et les spéculations auxquelles il donne naissance.

L'appréciation de la théorie de ce parallélisme étant une question de faits, signalons quelques recherches empiriques qui concernent cette théorie. On a comparé le poids du cerveau chez l'homme et divers animaux et on a relevé un rapport entre ce poids et l'intelligence. De la même façon et avec le même résultat

on a comparé des personnes plus intelligentes à des personnes moins intelligentes. Des expériences sur des animaux nous ont appris que l'extirpation du cortex a comme conséquence la disparition des expressions et des mouvements qui sont considérés comme une indication de la vie psychique la plus élevée. Et pour les hommes on a trouvé des cas de troubles mentaux correspondant à des lésions cérébrales. Ici nous pensons aux recherches, chronologiquement les premières, de Broca et de Wernicke, à l'aphasie motorique et sensorielle. Mais passerons aux détails de ces recherches pour relever:

1. que cette théorie, comme théorie d'un parallélisme général concernant tous les phénomènes psychiques est une généralisation. On n'a pas encore trouvé pour chaque phénomène psychique le processus cérébral déterminé correspondant. Loin de là. Quant à cette généralisation, on peut se demander si elle est légitime ou non, car la science connait des généralisations légitimes: pensons à l'induction amplifiante. Mais, dans le cas présent, on s'est donné à la généralisation bien vite. Nous savons que ce parallélisme a été lancé comme une théorie générale quand des recherches minitieuses n'avaient pas encore eu lieu, et lancé dans une forme spontanément interprétative. Ainsi nous rencontrons au dix-huitième siècle le matérialisme physiologique de la Mettrie, l'auteur de «l'homme machine». Il doit exister une raison spéciale pour cette généralisation. Mais nous n'allons pas approfondir cette question-là. Nous nous contenterons ici de déclarer que ce matérialisme physiologique est l'effet d'un jugement unilatéral. Une fois reconnu comme tel, ce matérialisme physiologique est inoffensif; d'autre part il garde sa valeur de méthode pour des recherches de l'ordre physiologique. — Revenons au parallélisme. Comme théorie générale il est une hypothèse, qui n'est pas vérifiée, mais, si l'on donne à cette généralisation la signification d'une hypothèse de recherches, elle a une valeur incontestable.
2. Une vérification intégrale est d'une grande difficulté, car, pour bon nombre de phénomènes psychiques nous devons nous attendre à un parallélisme comportant des processus fort complexes et très répandus dans le cerveau.
3. Il faut se rendre compte que ce parallélisme, en tant que

«empirique», n'implique pas une relation causale, mais donne une corrélation.

4. Les recherches empiriques dans le domaine de l'anatomie et de la physiologie cérébrale ne peuvent nous indiquer que des conditions cérébrales concomitantes avec le phénomène psychique en question, de telle sorte que si ces conditions ne sont pas réalisées, le phénomène psychique ne sera pas réalisé non plus. Dans ce sens la physiologie cérébrale nous indique — et peut nous indiquer de plus en plus — des conditions corporelles qui sont des «conditiones sine qua non» pour le phénomène psychique, mais il faut se rendre compte que ces phénomènes cérébraux ne sont que des «conditiones sine qua non». Si l'on juge autrement, on dépasse la connaissance empirique et on s'avance sur le domaine de l'interprétation. Nous n'avons pas du dédain pour l'interprétation explicatrice qui est ici de l'ordre métaphysique. Mais ce n'est pas notre problème d'à présent, laissons donc la métaphysique, puisque le but de notre article est autre.

Quel est ce but? Je veux soutenir la thèse que le parallélisme dont nous avons parlé jusqu'ici n'est qu'une élaboration spéciale et souvent plus ou moins hypothétique d'un parallélisme plus ample. Ce dernier est incontestable, et il est fondamental pour notre conception du psychique. Nous rencontrons ce parallélisme quand nous nous rendons compte de la voie par laquelle nous parvenons à la distinction entre le monde physique et le monde psychique.

Avant de continuer, il convient de nommer Theodor Ziehen, parceque ses livres «Psychophysiologische Erkenntnistheorie» et «Die Grundlagen der Psychologie» m'ont ouvert les yeux sur ce parallélisme plus ample et sur son importance fondamentale pour une juste conception du psychique. Malheureusement, à mon avis, par la faute de son positivisme Ziehen ne tire pas les conséquences de son point de départ, mais je lui suis reconnaissant de l'impulsion que la lecture de ses livres m'a donnée.

Prenons comme point de départ une perception quelconque, une donnée immédiate de la conscience, par exemple la perception visuelle «cette lampe». Je nomme cette perception une perception primordiale, puisqu'elle est primordiale pour notre

«ratio cognoscendi». Cette perception, comme un des contenus de la conscience, est dans l'espace en dehors d'autres contenus, en dehors de la perception visuelle «ton corps», en dehors de la perception visuelle «mon corps à moi». Toutes ces perceptions sont des contenus de la conscience [1]). Jusqu'ici nous n'avons encore aucune raison de les appeler physiques ou psychiques; elles ont encore leur caractère primordial de neutralité psychophysique.

Mais bientôt, par des expériences qui sont très communes, des expériences faites par tout le monde, nous devons envisager une telle perception *sous deux aspects*. Aussitôt nous obtenons la distinction entre «physique» et «psychique».

Premier aspect. La perception «cette lampe» est dépendante de mes yeux ouverts. Si je ferme les yeux, elle n'est plus là. Elle est dépendante des mouvements de mon corps. Si je détourne la tête, je n'ai plus cette perception. Si je reprends la situation précédante, de nouveau le contenu est là. Par des expériences plus détaillées, j'apprends que la perception est dépendante de ma rétine, du nerf sensoriel, de l'état de mon cerveau. Bref, elle est dépendante de mon corps et de l'adaptation de mon corps.

Il n'est pas nécessaire de voir immédiatement cette dépendance comme une relation causale; il est préférable de constater d'abord que nous avons ici une corrélation entre un contenu de la conscience (la lampe) et un autre contenu de la conscience (mon corps et son adaptation). En effet, mon corps est, de même que la lampe, une perception primordiale. Il s'agit donc d'une corrélation, d'un parallélisme entre deux contenus de la conscience. J'appèle cette corrélation *l'aspect physiologique*, l'aspect de l'adaptation du corps.

Deuxième aspect. La perception «lampe» n'est pas exclusivement dépendante de l'aspect physiologique. Il est bien possible que demain, aussi bien adapté que maintenant, je n'aie plus la perception «lampe». Nous jugerons alors qu'on aura transporté

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[1]) On a critiqué ce terme «contenu de la conscience», et la critique est légitime quand on prend la conscience comme une entité *qui a des contenus*. Une telle conscience n'existe pas. Ces contenus, c'est nous comme sujets qui en sommes conscients. Mais puisque ce n'est pas le moment d'insister sur le sujet qui a conscience, je fais abstraction du sujet. Tout de même, précisement parce qu'un sujet qui a conscience est sous-entendu, je préfère le terme «contenu de la conscience» au mot «contenu» tout court.

la lampe pour faire une réparation, pour en faire usage dans une autre chambre, etc.

Bref, la perception peut se perdre, nonobstant le facteur d'adaptation physiologique. Ainsi nous devons admettre que cette perception est dépendante en outre d'une autre connexion, celle des changements de la nature. C'est *l'aspect physique*.

La perception, conçue comme dépendante de l'aspect physiologique, est la perception jugée *subjective*, c.a.d. jugée comme expérience individuelle, comme expérience d'une qualité dite *psychique*.

La perception, enchaînée dans la deuxième connexion, c'est la perception *objective*. Quel est le sens du mot «objectif»? Contentons-nous de dire ici: ce mot signifie simplement que l'on a fait abstraction des vicissitudes de l'adaptation physiologique.

Nous avons appelé la perception, quand elle n'est pas encore comprisé dans ces deux connexions, perception primordiale, c.a.d. perception qui a encore le caractère de neutralité psychophysique. Cette expérience primordiale n'est ni physique ni psychique. La physique et la psychologie acquièrent leurs objects respectifs dès que l'expérience primordiale est liée avec ces deux connexions.

Bien-entendu la perception «notre corps» est de même prise dans ces deux connexions. Nous avons la perception «corps», jugée *subjective* et *jugée objective*, selon la connexion respective.

Et maintenant nous pouvons préciser: la perception, comme dépendante de la connexion physiologique, donc perception *subjective*, est dépendante du corps (adaptation du corps), mais du corps pris dans la connexion physique, comme perception objective.

La perception, sous l'aspect physique, est l'objet des sciences physiques et elle subit, en passant dans les théories physiques, de grands changements. Nous rencontrons, chez Démocrite déjà, plus tard chez Locke, la distinction entre qualités primaires et qualités secondaires. La physique fait abstraction des qualités secondaires comme qualités, qu'elle juge être subjectives. Bref, la perception primordiale devient dans les conceptions physiques une tout autre chose. Mais nous pouvons laisser de côté ces transformations dues aux sciences physiques.

Plus intéressante pour nous, dans le cadre de notre exposé, est la question suivante. La perception, jugée *subjective, psychique,* est-elle aussi l'objet d'une réflexion altérative. Certainement, il y a changement d'espace. La perception primordiale (cette lampe) est hors de cette autre perception primordiale (mon corps), mais la perception (cette lampe), comme jugée subjective, est localisée quelque part dans mon corps, c.a.d. dans la perception (mon corps), jugée objective. Il est clair que nous devons opérer cette localisation. Quand les paupières sont fermées, la perception n'est plus là; elles sont comme un rideau que nous pouvons fermer ou ouvrir, et comme — le rideau fermé — il n'y a plus de perception, on peut, par postulat, maintenir la perception (objective) devant le rideau (réalisme naïf); mais la perception subjective doit être placée de l'autre côté du rideau, puisque, les yeux fermés, elle n'est pas là. Pour que la perception visuelle soit donnée, il faut que les paupières soient ouvertes, la rétine intacte, le nerf optique non coupé, et ainsi nous voyons cette perception comme le maillon dernier d'une chaîne, qui nous mène de la surface de notre corps, de la périphérie, vers l'intérieur.

Les données immédiates sont des contenus dont nous sommes conscients, et dans ce sens nous pouvons les appeler tous, sans exception, des contenus de la conscience. Ce serait commettre un faux départ que de commercer par attribuer à la conscience cette spécification par laquelle les données immédiates seraient considérées comme des expériences individuelles, c.a.d. des expériences subjectives, de caractère psychique.

Certainement elles sont aussi psychiques, mais si on les considère commes telles, on les a prises déjà dans la connexion physiologique. Dès qu'un contenu de la conscience est jugé comme expérience individuelle, c.a.d. psychique, c'est le corps et son adaptation qui sont dans le champ de notre attention, et alors on est tenté d'enfermer tous les contenus primordiaux dans un des contenus, dans notre corps. Si l'on s'abandonne à cette tentation, on crée un «vitium originis», qui mènera à des problèmes qu'on peut plus ou moins escamoter, mais non résoudre.

Il faut voir simultanément les deux connexions dans lesquelles les perceptions primordiales sont prises.

«Notre corps» peut être une perception primordiale, il peut

être une perception subjective, il peut être une perception objective, cela dépend de l'aspect. Une perception primordiale quelconque, qui est spatialement extérieure à une autre perception primordiale (en l'espèce: notre corps), ne saurait être localisée dans ce corps, sous aucun des trois aspects considérés. On peut faire cette localisation, et même on doit la faire, dans le seul cas où l'on prend cette perception quelconque *sous l'aspect de la subjectivité* et le corps sous l'aspect de l'objectivité.

La conception du psychique est fondée sur un parallélisme entre des contenus de la conscience, d'une part, et des changements effectués par notre corps et dans notre corps, d'autre part. Il y a une grande quantité de changements corporels qui ont été le point de départ de l'idée du psychique. Bien-entendu, il y a beaucoup d'autres changements dans notre corps qui n'ont pas joué ce rôle et qui peuvent être indifférents. Mais, après que la conception du psychique est formée par ce parallélisme, nous pouvons demander et chercher méthodiquement quels autres changements du corps correspondent à des changements dans la vie psychique. Et alors nous rencontrons ce parallélisme mentionné au commencement de notre exposé, le parallélisme dit empirique, qui affirme qu'à tout phénomène psychique correspond un processus cérébral tel qu'à chaque changement du premier répond un changement du second. Dans ses détails d'un caractère plus ou moins hypothétique cette théorie ne donne que des élargissements du parallélisme que nous avons désigné comme fondamental pour la conception du psychique. Si le physiologiste s'abstient de détailler, car en détaillant il relève des corrélations plus ou moins certaines qu'il ne nous appartient pas de juger, s'il affirme en général que tout phénomène *psychique* s'accompagne d'un phénomène physiologique, nous pouvons sympathiser avec son attitude, car la conception du psychique présuppose un accompagnement des phénomènes physiologiques.

Une fois l'idée du psychique formée, on en vient à des conceptions consécutives et explicatives, comme celle de l'âme par exemple, et alors on oublie facilement ce qu'il y a à la base de l'idée psychique.

Pour évaluer la signification philosophique du parallélisme

il faut se répéter que les données immédiates de la conscience *sont à la base de toute notre connaissance* et qu'on ne doit pas identifier ces expériences primordiales avec des expériences *psychiques*. Tant qu'on ne s'est pas rendu compte de la conception du psychique, il y a grand danger qu'on commette la confusion. Et alors on se figure que la théorie du parallélisme, qui vise la vie psychique, désigne de ce fait les données immédiates de la conscience. C'est une grande erreur avec des conséquences philosophiques néfastes. Cette erreur — fortifiée par quelques autres malentendus — a mené au matérialisme métaphysique. Mais signalons plutôt, en général, que par cette erreur l'importance philosophique du „parallélisme empirique" („que tout phénomène psychique s'accompagne d'un phénomène cérébral") a été exagérée chez beaucoup de ses défenseurs, de même que chez des antagonistes.

LANGUAGE AND PSYCHOLOGY

BY

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The psychology of language received a new impetus, thanks to the work of G. Révész. His fertile and original mind tackled very old problems with new methods, and this led to surprising and stimulating results. Révész's work in the field of psychology of language bears the mark of his knowledge and experience in other fields of psychology, such as the theory of perception, the psychology of art, the theory of talent, the animal psychology, which, however different they may be, are mastered by him as a specialist. By this great versatility also Révész has been able to render such important contributions to the development of psychology. None of the fundamental problems of psychology can be solved with the aids of only one of its branches. The following article is based on this conviction, formed and strengthened in years of close contact with Révész, as a pupil and a co-worker.

Inhabitants of a small country with a language strongly deviating from the languages of its surrounding countries, as is the case with the Netherlands, inevitably come in contact with all sorts of linguistic problems which inhabitants of greater language areas do not know, or not to the same degree. These problems increase in number and importance as the means of communication become better, and the traffic to and from foreign countries becomes easier. If the inhabitants of such a country appreciate contact with one or more great culture areas, they have to provide themselves with at least a reading acquaintance of one or more foreign languages. One of the most important problems for those who want to, or have to, come in more active contact with foreign countries is the translation and the translatability of the expressions of their own language into other languages. The following treatise must be seen against this background of personal experience and personal situation.

The formulation of its results forms an essential part of the task of science. Formulation means: express in language, i.e.,

make accessible to others. Scientific knowledge is essentially communicable; that which is not communicable falls outside the scope of science. Every science aims at building up its own system of conceptions with which to describe the field of reality investigated by that science. Some conceptions appear to be more suited for a certain science than other conceptions. So it is not immaterial which conceptions a science uses for the description of the phenomena investigated by it. The progress of science exists mainly in the discarding of obsolete, now useless conceptions, and the formation of new more adequate ones. Although the most striking examples, as illustration of this postulation, are to be found in the history of the natural sciences; the relatively short development of psychology already also offers some materials. Notions such as association, element, apperception, are since long past their flourishing period, and many investigators regard them as useless and misleading nowadays.

When we may say that the present knowledge of the natural phenomena is more thorough and more extensive than that of former generations, it is due to the fact that our present terminology is much more efficient thans theirs. The terminology of the present natural sciences is much further removed from the ordinary colloquial language than that of the former. A specific physical professional language has developed. This professional language is the necessary condition for the progress of science. It is created by the scientists because they cannot do without it. The professional language deviates more from the colloquial language as the science progresses.

One of the most unwanted side effects of this course of affairs is that on the one hand such a field of science becomes more and more inaccessible to the layman, while on the other more persons must be regarded as laymen. Nowadays not only uneducated persons must be considered laymen with respect to modern physics. Those with a good general education are also in the same situation. Even people with a university education, such as jurists, economists, biologists, *etc.*, are laymen with respect to physics. Mutatis mutandis this not only holds for physics, but also for many other branches of science.

When we apply this seemingly superficial criterion of the discrepancy between colloquial language and professional language on psychology, then it seems as though we can only

reach the conclusion that psychology has but little developed as yet. It cannot indeed be denied that the general reader can much more easily follow nearly every psychological treatise than *e.g.*, one on quantum mechanics or on comparative physiology. In psychology an own professional language is only in its initial stages, and even on this there is little agreement among psychologists. It is true psychological works are often unclear, but this unclearness is not a consequence of the use of a professional language which is clear for specialists, but of a lack of lucid argumentation and command of the colloquial language, respectively. In other words, it does not follow from the nature of the subject dealt with, but from the inability of the authors. Psychology has no professional language consisting of clearly circumscribed, generally accepted conceptions; it uses mainly the ordinary spoken and written language. But is the above-mentioned conclusion, drawn from this fact, justified? Is it justified to postulate that psychology has not yet sufficiently developed to form its own professional language?

It does not appear impossible to us that the situation pictured here also has other causes connected with the special relationship existing between language and psychology. In the following we shall try to take a closer view of one or two of the aspects of this relationships.

The basis of the name-giving, conception-formation, respectively, in the natural sciences is a principle which is so evident that it is seldom explicitly formulated. It is the principle that the object is not changed, let alone created by the name-giving. That which is given a name, is formulated, respectively, was already in existence — as a thing, as a relationship, as a law — but it had not yet been discovered. The term *discovery* is generally adopted in the natural sciences: a new island, a new star, an unknow law of nature can be *discovered*, according to the colloquial language. They were already in existence, and are not changed because one or another scientist now discovered their existence and made this known to others by name-giving or formulation. Newton did not *bring about* that bodies were to behave with respect to each other according to his law. They had always done so and would always do so, even though the law of gravity had never been discovered, *i.e.*, never been formulated. The efforts to test the formulated hypotheses by means

of the experiment fits also in the same line of thoughts. Physics bases itself rightly on the standpoint that not the opinion of the investigator settles the matter, how great his prestige may be, but the facts only. These facts follow from the laws of the processes investigated.

Strictly speaking man does not cause these facts; in the experiment he only creates the proper conditions for their occurrence. For example a chemical experiment is taken: materials are added together, which according to their own structure and without further action of the investigator, compound to a new material. To paraphrase Kant's well-known words: "in the experiment man enables nature to answer questions". It does not fundamentally depend on man how that answer turns out. With the natural phenomena he is essentially a spectator, especially with the natural phenomena tested in the experiment.

In so far as the facts are the same the observations can also agree, and the communications with respect to them can be identical. Hence it is that physics has an *international vocabulary* at its disposal. It does occur, of course, that there is no agreement in the terminology when new fields are investigated, that the same phenomena are explained in various ways, and that contradictory hypotheses fight for priority. But it is not in the least necessary that these differences in terminology coincide with the language boundaries. Investigators from various countries can use a common terminology, and country-fellowmen may use conceptions completely deviating from each other. However, such a situation is considered by all parties concerned as one for the time being. The unity of terminology is considered guaranteed by the uniformity of the facts.

The ideal of the experimental psychology is in principle the same.

The introduction of the experiment into psychology was nothing else but the introduction of the physical methods into psychology. Here also the same habitus is required of the investigator: he has to limit himself to the creation of the proper conditions under which the processes to be investigated can run their course. Human nature has to show itself here, has to answer the questions put, just as the dead material has since long done so docilely. But here remarkable complications occur in various respects.

In the first place it appears that the creating of the necessary conditions has to be done quite differently than in physics. It is not sufficient to bring man in a physically determined situation, and then only to observe his reactions. People can have different views on the same situation, and their reactions are to a high degree dependent on these views. This phenomenon of the view on, or the significance of the situation, respectively, has no parallel in the physical world. Hence that in the psychological experiment something is done which is unnecessary in any physical experiment. The 'reagens', man, is spoken to and certain directions are given to him in order thus to obtain in various subjects at least comparable views on the situation and comparable reactions to the situation.

There is hardly any psychological experiment in which not one or other *instruction* is given. This is something we find nowhere else. The language is not only used to *describe* the phenomenon, respectively to communicate it, but constitutes an essential condition for the *evocation* of it. And which language is used here? Not one or other professional language, but the ordinary spoken language of the country in which the investigator is working. When an experiment is repeated in another country one of the conditions has to be changed at once. When the languages involved are highly different, these changes will often have to be considerable. So here is a first and preliminary indication of a relationship between the scientific psychology and the colloquial language, of which no parallel exists in any of the natural sciences. Moreover, it should not be forgotten that the rôle of the language in the psychological investigation is not limited to the instructions. The experiment is preceded as a rule by an invitation to take part in it, to be present in the laboratory at a fixed time, and sometimes an extensive explanation of the meaning of this investigation, *etc.* It would be impossible to get any human being as a subject in the laboratory without the language, and this leads to all kinds of complications which make it quite understandable that rats are sometimes preferred.

The language has to guarantee for a great deal the uniformity of the experimental situation, which should form again the basis of the comparability of the results and of the unity of the theory and conception formation. But this guarantee is very

deficient and incomplete when we deal with persons from different language areas, and hardly sufficient with respect to persons with a common mother tongue. It is generally known, at present, that the mastership of the language, active as well as passive, is highly different from individual to individual. One of the most important causes of the occurrence of these differences is the fact that the lingual surroundings are very different, not only from country to country, but also within one country for various groups of the population. These differences will manifest themselves in many ways in the experimental situation, and disturb its uniformity. In practice all this is insofar taken into consideration as certain demands are made upon the test persons with respect to their education, sphere of interest, *etc.* But to make these demands in practice often results in an unwanted selection for lingual milieu. When this is impossible as a result of the nature of the problem investigated, the above pictured difficulties manifest themselves in great force. The standardisation of intelligence tests is mentioned as an example. Here the sensitivity for milieu influences appears very clearly, especially of the verbal tests. These difficulties come even more to the fore when the intelligence tests are translated into other languages. This is to some degree possible when the translation has to be made in a related language, *e.g.*, from English into French or Dutch. It is almost impossible when the translation has to be made into Chinese or into a Bantu language. Then a new test has practically to be constructed by which the comparability of the results all but disappears.

It clearly appears, even from these few examples, that the language, *i.e.*, the ordinary colloquial language, plays a predominant rôle in scientific psychological research. It is a condition for the evocation of the phenomena to be investigated, it works as a differentiating factor on the human conception of the situation and thus on the human achievements.

It is useful, in connection with the problem of the relationship between language and psychology, to point to another consideration. Our psychological knowledge is not only found, extended and consolidated in the science of psychology. There is another field forming a reservoir for psychological notions, *viz.*, literature. It is true literature uses partly a special terminology (as a rule there is a difference between the written and

spoken language), but this terminology is very close to the colloquial language, in any case in comparison with any scientific professional language. In literature much can be found which is of value for psychology. Our psychological knowledge is enriched by biographies, novels and poems. It is frequently the authors even who find the most adequate formulations, not the psychologists. If this is no illusion, if Shakespeare, Montaigne, Pascal, Dostojewski, Proust indeed succeeded in discovering and formulating psychological truths, then we have to accept that the normal language — and not only a professional psychological language which is still to be created — is suitable to express important psychological conceptions. Apparently the language is suitable to give an adequate description of the psychic processes, at least when it is used by an artist, that means, used in an optimal manner. So the system of conceptions of the normal language is not quite insufficient to bestow justice to the object of psychology. In any case there exists at the niveau of the description an undeniable affinity between the object of psychology and the normal language. But, whoever accepts this — and it cannot be denied — has to face an imperative problem. For until now we discussed the relationship between the language and the object of psychology. However there is not only *one* language, there are a great many languages. Which language has this special relationship with the object of psychology? All of them? How is it in the last case with the mutual differences between the languages? There are considerable differences between the languages, and this cannot too often be repeated. This already holds for related languages — such as English, French, German and Dutch — and especially for languages with a quite different structure, such as *e.g.*, Chinese.

An analysis of these differences cannot yet be given — this is a task for the future. Lee's article is a remarkable effort in this direction [1]). Although we cannot yet define precisely the nature of these differences, their existence comes very clearly to light in the difficulties of a translation. Even with related languages it is very difficult to find adequate expressions, to transpose

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[1]) *DLee*, Lineal and Nonlineal Codifications of Reality. Psychosomatic Medicine, XII, 1950.

the idiom of the one language into that of the other. This difficulty becomes all the greater as we deviate more from a simple summation of objects, move more to the appreciation or the conception of the objects, are more removed from the objective, and approach more the subjective.

In this connection especially the following is of importance in psychology. Most people speak one language only really fluently — their own language, their mother tongue. They are justly content with it. They can use this language in all situations in which they are in need of it. It does not let them down. They can say in their language all they have to say. However, it is possible to express things in another language for which there exists no equivalent in their own language. But the normal users of the language don't perceive this, don't feel it as a lack. There are of course those — often people with a very great mastership of the language — who complain of the inadequacy of the language, and who are of the opinion that the use of the language always violates the intentions of the speaker. They are in the minority, and moreover, they do not mean that a language other than their own could render better services. Their objections do not refer to their own language only, but to the language as such. As a rule they do not consider their own language so poor in comparison with other languages.

If, generally speaking, everybody's language comes up to his desires, while the various languages can only fulfil different desires, then it cannot be but that the desires are also different. For most people that which cannot be expressed in their own language does not exist. In the strict sense it is beyond their mental horizon. Conversely: everybody thinks to be able to express everything that exists for him in his own language. There is an unreasoned, unscientific conviction of the potential adequacy of the language.

Judging from the achievements of the most talented users of the language this conviction is not incorrect with respect to the psychological description. What consequences result from this for psychology? To answer this question we shall orient ourselves to one of the forms of literature — poetry. Exactly in poems a reflection is often found of moods, sentiments, emotions, passions, which no psychologist wants to exclude from psycho-

logy. The poem may be considered as a giving of form to the experiences of the poet. A good poet is characterized by his mastership of the language. His fellow-countrymen consider his poems as adequate expressions of just these inner experiences. Here therefore the language fulfils one of its functions in an optimal manner. At the same time this language is the most idiomatic; all kinds of phrases only characteristic of this language, nuances, parables, metaphors and allusions play the greatest rôle. Where the language realizes its psychological potentialities maximally it shows most clearly its own character, and then the difficulties of translation are the greatest.

It is desirable to stop at this for a while. There where the inner experience is most pregnantly formed and expressed in language, it obtains at the same time the indelible mark of a certain idiom. It follows from this that it would be impossible to express this experience in another language. Consequently we find that well-translated poems are scarce, and further that they are re-creations rather than translations, *i.e.*, new poems in another language in which certain aspects of the original poem, but never all of them, are evocated. This is quite understandable. The poet uses all means of the language, also *e.g.*, the sound. Whatever may remain constant in the translation from one language into another — certainly not the sound. Even in the most excellent translation of *e.g.*, an Italian poem into English other sound material must be used and therefore other things are pictured and evocated. It is not possible to duplicate a poem with other words in the same language, neither is it possible to translate it completely adequately into another language.

If we direct ourselves to the psychic experience of which the poem is the expression and form giving, then we must ask ourselves for the reason why this experience possesses apparently such a structure that it can be expressed only in one language. In my opinion only one explanation can be given here, namely, that the language can be so suitable as expression of the psychic experience because this inner experience itself is structured in a certain manner under the influence of the language. The language is therefore so suitable as an adequate expression of the psychic life, because it not only expresses but also moulds and forms that which is expressed.

This explanation is plausible for the language exists before the individual. Everyone learns it from his surroundings. The changes which a person can effect in the language of his country are minimal. An individual language is an impossibility, as such a language cannot fulfil a communicative function. Therefore it is impossible that an adequate expression of the psychic experience is achieved because the individual forms the language. But then the choice should fall on the other alternative: the language forms the individual psychic life.

We can only form a gross idea on the manner in which this happens. It is useful perhaps to make a comparison with another product of culture which exerts also a great influence on man, namely the large town. A large town forms the life of its inhabitants. It gives contents and direction to their activities. It procures them the ways, which are considered the only possible. If I want to go to the drugstore on the other side of the block in which I am living, I walk round the block. There would be the possibility of going through the houses, and climbing over various fences. I could reach the back-door of the drugstore in this way. However this possibility is not realized under normal circumstances. It would not even occur to me. The town provides the drugstore and the way to reach it. More generally speaking the town provides the goals and the ways to reach them. The town is an historically grown agglomeration of streets and buildings. It is situated in a certain landscape possessing certain natural characteristics, so that the plan is determined on great lines. Various cities however with different plans could have been possible on the same site. The vicissitudes of history are responsible for the present form of the town. But the inhabitants do not think of that. The town is for them their "natural" surroundings. It was there when they were born, and they are not aware of the historical fortuity of its present shape. They feel at home in it, and conform to its demands as to unwavering commandments. They know their way, mostly without even being able to design a map correctly.

It is clear that this situation shows much resemblance to that average user of the language. His language is one out of many, historically grown, and as such to a certain degree fortuitous. But for him it is the only one, the milieu in which he lives and thinks. The language provides all he wishes to say and how

he has to say it, the direction, the contents of his act of speech. He is not aware that other aims and means could have been possible had the language grown otherwise. He is a citizen of the city of his language, and in no other language would he ever be able to move with the same security and agility. He is conditioned by his language, and all his thoughts bear its mark. Just as the citizen knows his way around but could not draw a good map, so the user of the language uses the language, the grammar of which he does not know and which he certainly could not compose.

In another respect still this image can help us to visualize the influence of the language on the individual. It would appear as though the members of a language community would have to show under this influence a high degree of psychic conformity, as though interindividual differences would stand no chance of developing. But this is not the case. The town offers an endless diversity of ways and destinations. So the language also offers an endless diversity of possibilities, of which every individual can realize only a limited number. Just as the town is the world for the citizen, the language forms the world for all of us. It offers us the entry to reality, and is not less rich than our reality.

So in a certain sense the language imposes limitations to those who employ it. But these limitations are the condition for every form of activity. They can *e.g.*, be compared to those inherent to observation. When we observe an object, this object is given in its entirety. But we see it always from one side only. A complete observation, in which simultaneously all aspects of the object are given, *i.e.*, in which we would place ourselves simultaneously on all possible standpoints, is impossible.

Thus it is also with the language. We can enter the world as a whole via every language. However, every language offers a certain view on this world. Some of these views are all but similar, others diametrically opposed. A language without such a view, however, is just as impossible as an observation without a standpoint.

Our view on the world, *i.e.*, our world, is therefore to a high degree determined by the language we speak. Well, man lives, acts, develops in interaction with his world. His behaviour is attuned to the world in which he lives. His personality is formed

by contact with his world. Apparently the language will also exert influence on the personality.

Once again it is stressed here that it is not permitted to conclude from this that all members of a language community would possess the same personality structure, or the same view of life. If the language exerts an influence, this is so radical that is goes deeper than the differences in view of life — which, as a matter of fact, use to reflect themselves in the use of the language. It offers the common fundament, on which basis the differences can only become possible. Even the specific structure of the typological differences is influenced by the common language, even though these differences themselves were brought about by biological causes and even though they manifest themselves also in a way independent of the language. Insofar as such differences in the human existence develop into various ways of choosing position with respect to the world, so their common relationship to the language is undeniable.

In my opinion insufficient attention is in general paid to the influence of the language on the development of the personality. This is perhaps partly due to the fact that in present psychology so much importance is attached to the unconsciously occurring processes. According to the current notions the unconscious is alingual. None the less we should be careful here. For he who attaches great importance to the unconscious cannot deny the great importance of the emotional life, also in its conscious manifestations. At present it is possible to characterize nearly all important currents in psychology as "dynamic", *i.e.*, they can be considered as occupying themselves with the motives and forms of the human activity. This means that none of them neglect the emotional factors; on the contrary, they place these in the foreground. If nowadays it is said of a psychological current that it is "intellectualistic", this is mostly meant as a reproach that such a current does not grant that dominating place to the emotional life to which it is entitled in the opinion of most of the psychologists.

We perceive the influence of the language perhaps nowhere so clearly as in the emotional sphere. Every language has a great many expressions and phrases to express emotional situations and experiences. This richness can in itself perhaps be considered again as an indication for the importance of the emotions.

However, we can leave this out of consideration here; we only want to point out that such words and expressions are the most difficult to translate. I do not only think of literary products now, but especially of the common-place words which we use in daily life to make our emotional reactions clearer to ourselves and to express them to others.

For example, the English word "to hate" is used in numerous different contects. The Dutch word "haten" can be used as a translation in only a few of these contexts. In a great many cases in which the expression "I hate him (her, it)" would be in its place, the corresponding Dutch expression „ik haat hem (haar, het)" would be utterly misplaced. Then another formula should be chosen, another word or a circumscription used. Nothing is more generally known than this: every high-school boy knows it. But is is psychologically interesting that apparently the English user of the language has the same term for a great many emotional situations which have for the Dutch a strongly varying nature; in the leading-strings of his language (and of his socio-cultural surroundings) the Englishman treats all these situations alike. It is important here that these and numerous other expressions for emotional states are used in a quite spontaneous, immediate, direct way, and that they are considered by the user to offer an *adequate* reflection of his emotions. One language apparently differentiates in a different way from the other; it joins and separates otherwise, but the users of these languages are thereby not impeded in the spontaneous expression of their emotional states. They are guided apparently by their language in bringing to the foreground and neglecting certain aspects of these emotional states; seemingly the language exerts an influence on the experiencing of these emotions.

What is argued above becomes still clearer when we take into consideration that language and user of the language are involved in a pattern of social relationships, of socially accepted and socially rejected forms of behaviour, and that in daily life the use of the language rarely fulfils a descriptive, but mostly an actional function. Activities and impulses to activity, current in the socio-cultural milieu of the speaker, are connected with the expressions used. Many authors have *e.g.*, pointed to the discovery of the young man who feels himself attracted to a

girl, and now suddenly realizes that he loves her. Under these circumstances the word "love" is not only the expression of a state of mind, but also, perhaps even more so, the *identification* of it. But this identification forms only one aspect. A whole pattern of ways of behaviour, of expectations, of commandments and prohibitions, of norms and ideals, is connected with the term of language, so that to the word "love" a function is given which is from a quite different nature than a statement or a registration. For our subject it is of importance that this social pattern differs from society to society; it is not identical even for all groups of the population within the same country. So the emotions, impulses and expectations connected with such emotionally loaded terms will diverge for different individuals, depending on the social situation. Yet they all use it; still it is for the one not less adequate than for the other. How can this otherwise be explained than by the assumption that the state of mind is influenced by the words in their socially and historically determined significance?

No community can exist without a certain conformity. Every community of a certain strength has a language at its disposal. There are all kinds of languages, just as there are all kinds of communities. Peoples, classes, professions, *etc.*, have a language of their own. Especially when we acknowledge the influence of the language on the psychic life and thus on behaviour, we can understand and evaluate this phenomenon. It appears then that the language is one of the most powerful means to effect the necessary conformity, as well in psychological respect, between the members of a community. This necessary conformity is no other than that the people "understand" each other. If there is no possibility for an understanding, the basis of the community is at a loss.

It will be necessary that in psychology the influence of the common colloquial language on the psychic life is realized more than is the case up to the present. Only when this is taken into consideration will it be possible to penetrate to the most fundamental and most universal laws of the human behaviour, and only then will a really general psychology become possible.

DAS BEWUSSTSEIN UND DAS TRANSZENDENZPROBLEM

(Erleben, Denken, Wollen und das Bewusstsein)

von

TH. ERISMANN (INNSBRUCK)

Mir scheint, dass diese vier Begriffe nur in Gemeinschaft behandelt und geklärt werden können: so eng ist ihre innere Durchdringung. Gewiss ist die Trennung des Verschiedenen stets entscheidend für die Klarheit wissenschaftlichen Denkens; nicht weniger bedeutsam aber ist die Erkenntnis der tieferen Verwandtschaft, dort, wo sie sich hinter aller Verschiedenheit verborgen hält und von da aus die Grundstruktur des Oberbaues bestimmt. Der Hauptfortschritt der vergleichenden Tierkunde ist erst durch die Erkenntnis des Uebereinstimmenden auch noch in den scheinbar verschiedensten Tierformen erlangt worden. So scheinen uns auch 'Erleben' und 'Denken' grundverschieden; diese Verschiedenheit soll unangetastet bleiben, — aber gewährt uns ihre *Verwandtschaft* nicht vielleicht noch tiefere Einsicht in ihr Wesen als ihre Verschiedenheit?

Was diese anbetrifft, so scheint sie vor allem darin zu liegen, dass der im Denkgebiet entscheidende Begriff der Wahrheit im Gebiet des blossen Erlebens unanwendbar ist. Denn das Erleben, — hört man sagen, — meint nicht etwas, behauptet nicht etwas, hat nicht etwas über es selbst Hinausgehendes zu seinem Objekt, — sondern es 'erschöpft sich in seinem Eigensein'. Das Erlebnis des Schmerzes, des Geschmackes, des Tones, der Helligkeit sind Letztgegebenheiten, aus deren Auftreten und Vorhandensein sich unser Erleben, sich unser Leben aufbaut. Sie bleiben, was sie sind, auch wenn ihre gedankliche Ausdeutung auf die ihnen in der Aussenwelt zugrundeliegenden Reizursachen hin so oder anders ausfallen mag. Selbst völliges Fehlen der Aussenreize, wie es Halluzinationen, neurasthenischen Schmerzen, subjektiv bedingtem Ohrensausen usw. eigen

ist, ändert nichts am Erlebnis des Schmerzes, der Gehörsempfindung usw. als solchem. Während der an sie sich anschliessende *Gedanke:* „Dem Erleben liegt eine Aussenweltsursache zugrunde", und eventuell noch darüber hinaus: „diese Aussenweltsursache ist von derselben Qualitätsart wie mein Erleben, d.h. Töne, Farben, Gerüche usw. sind als solche auch ausser mir in der Welt", — wahr oder falsch sein kann. — Man mag mit 'Erleben' den psychischen Akt, in dem uns der Schmerz, die Ton- oder Geruchsempfindung entgegentritt, bezeichnen; den Ton, die Geruchsempfindung selbst aber als das 'Erlebte' und so Erleben und Erlebtes unterscheiden. Aber im 'Erlebnis' bilden beide eine untrennbare Einheit: Es gibt kein Erleben ohne Erlebtes und kein Erlebtes ohne Erleben, ihr wesensmässiges Zusammensein ist das Erlebnis. Nicht ebenso scheint sich's beim Denken zu verhalten: Denn der Denkakt kann sein, ohne dass das Gedachte ist oder so ist, wie es gedacht wird. Und eben daraus erfliesst das Grundproblem aller Erkenntnistheorie, entsteht das gefährlichste Argument der Skepsis, das sich gegen die Möglichkeit aller Wahrheitsfindung richtet. Denn ist der Denkakt und das von ihm Gedachte seinsverschieden, sind beide zwei im Dasein von einander getrennte Vorgänge, oder Zustände, und ist uns nur das Denken unmittelbar gegeben, niemals sein Gegenstand, — wie könnte es dann möglich sein, dass wir jemals mit Gewissheit erfahren, ob das Gedachte wirklich so ist, wie es von uns gedacht wird? Mit anderen Worten, ob unser Denken wahr ist oder nicht?

Dieses scheinbar gar nicht mehr in die Psychologie, sondern in die Erkenntnistheorie gehörende Problem wollen wir schon hier ins Auge fassen, um sehr bald zu erkennen, dass sich die gewohnte Scheidung zwischen Psychologie und Erkenntnistheorie gar nicht durchführen lässt, ohne dass beide Wissenschaften an jenen entscheidenden Stellen, wo sie innig zusammengewachsen sind, Blössen, Unfertigkeiten, sozusagen eine Mangelerkrankung aufweisen, die sie als Wissenschaften unzureichend, ja geradezu lebensunfähig sein lässt. Denn enthält auch die Bezeichnung 'Psychologismus' einen vollberechneten Vorwurf, sofern der Psychologe die Eigenart der erkenntnistheoretisch logischen Fragestellung nicht erkennt, so darf er sich andererseits dort dieser Fragestellung selbst nicht entziehen, wo der Gegenstand seiner Untersuchung ohne deren Berück-

sichtigung schlechterdings nicht erfassbar ist. Und dies ist ohne Zweifel im Gebiet der Denkpsychologie der Fall: Wie könnte der Psychologe das Denken erforschen, ohne dessen Grundeinteilung in wahres und falsches Denken voll zu berücksichtigen? Ohne sich klar gemacht zu haben, wieso es so etwas wie 'wahres Denken' überhaupt geben kann? Die Antwort auf diese Fragen lässt sich aber nicht restlos geben, ohne den Zusammenhang des Denkens mit dem Begriff des Bewusstseins und damit auch mit demjenigen des Erlebens klargelegt zu haben.

Gewiss ist der Begriff des Bewusstseins ein Elementarbegriff, der sich nicht schlechthin auf andere Begriffe zurückführen und so definieren lässt. Aber auch Elementarbegriffe lassen sich in ihrer Eigenart durch Beleuchtung von verschiedenen Gesichtspunkten aus klarstellen. So der Begriff des Bewusstseins, wenn man sich fragt, ob ein Vorgang schlechthin und an sich ein 'Bewusstseinsvorgang' sein kann, oder ob er dies nur werden kann, wenn ein Bewusstseinsträger da ist, sodass der Begriff 'Bewusstsein' schlechthin seinen Sinn verlöre, wenn man Denjenigen striche, dessen Bewusstseinsvorgang der ins Auge gefasste Vorgang ist. — Zweifellos steht der Begriff des Bewusstseins in nächster Beziehung zum 'Wissen'. Von hier aus wollen wir an ihn herantreten, wobei sich's leicht herausstellt, dass das Wort 'bewusst, Bewusstsein' in verschiedenem Sinn gebraucht wird, was nicht vergessen werden darf, wenn auch der tiefste Sinn allen Anwendungsarten gemeinsam ist. — Zweifellos gebrauchen wir das Wort Bewusstsein oft sinnesidentisch mit Wissen. 'Ich bin mir der drohenden Gefahr bewusst' bedeutet nichts anderes, als dass ich von der Gefahr unterrichtet bin, von ihr ein Wissen habe. Und so auch 'Ich bin mir meiner Lage bewusst', 'Ich bin mir dessen bewusst dass Du anders denkst als ich, dass Du gegen mich eingestellt bist, dass ich ohne Deine Hilfe nicht zum Ziel gelangt wäre', — überall hier könnte das Wort 'bewusst' ersetzt werden durch 'ich weiss'. Bewusstsein ist hier gleichbedeutend mit Wissen, und dieses besteht in Denkakten, in Urteilen, in welchen ein Sachverhalt erfasst und festgehalten wird. Die, wie alle Urteile, mit Anspruch auf Wahrheit auftreten und mit Begriffen arbeiten, denen sie den Einzeltatbestand subsumieren. So hier, wenn die Behauptung aufgestellt wird, 'ich weiss', da wird mein gegenwärtiger Zustand unter den Begriff des Wissens gestellt. Gemeint ist nicht, dass ich will oder leide oder fürchte,

sondern dass ich *weiss*. Und darüberhinaus wird auch dasjenige angegeben, *was* ich weiss, und was nicht mehr in den Bereich meiner Eigenzustände hineingehört, sondern fremde Seelenvorgänge oder objektive Sachverhalte der Umwelt betrifft. — In diesem Sinne müsste man sagen: „Ich habe kein Bewusstsein davon, was mich umgibt", — wenn ich in einer mir absolut fremden Welt erwache, die in mir lauter mir bis dahin ganz unbekannte Eindrücke hervorruft. Was z.B. für das neugeborene Kind in der Tat so gilt, das ganz 'verständnislos' die ersten Sinneseindrücke aufnimmt, ohne sie zu erkennen, ohne sie schon bekannten Begriffen unterordnen zu können, da es solche Begriffe zu bilden noch nicht Gelegenheit hatte. In besonderen Zuständen, z.B. dem der Erschöpfung, können auch wir Erwachsene gleichsam seelenblind in die Welt hinausstarren, ohne die auf uns einwirkenden Eindrücke gedanklich zu verarbeiten, sie zu erkennen und übergeordneten Begriffen zu subsumieren. Dann kann man sagen: „Wir haben kein Bewusstsein davon, was sich vor uns abspielt; wir sehen zwar (sofern man darunter nur das Vorhandensein von Gesichtsempfindungen versteht), aber 'wir wissen nicht, was wir sehen'. Und wenn man unter Sehen auch das 'Sehen des Geistes', d.h. eben die gedankliche Verarbeitung der Empfindungen, das Wiedererkennen und subsumieren der Empfindungseindrücke versteht, so muss man sagen: In diesem Zustand der Seelenblindheit 'sehen wir nicht mehr'.

Aber auch wenn wir kein Bewusstsein davon haben, was wir sehen, — so hören wir, solange wir 'das Bewusstsein nicht verlieren', d.h. nicht z.B. ohnmächtig werden, doch nicht auf, unseres Erlebens bewusst zu sein. Hier ist 'Bewusstsein' offenbar in einem anderen Sinn gebraucht, nicht mehr als begriffliche Verarbeitung, nicht als Denken und Wissen, sondern schlechthin als *Erleben*. Und was bleibt auch noch in diesem blossen Erleben als wesentliches Merkmal zurück, was schon im Wesen des Wissens gelegen war?

Ist wohl ein Wissen denkbar, ohne dass *Jemand wäre, 'der weiss'*? Kann es ein „Wissen schlechthin für sich" geben — ohne einen Wissenden? Ohne ein übergeordnetes Sein, als dessen besonderes Teilgeschehen dieses Wissen anzusehen ist? Aus allem Obigen ergibt sich die ganz eindeutige Antwort: Da Wissen gleich Wiedererkennen, gleich Einfangen in schon bekannte

Begriffe, gleich 'geistiges Betrachten und Erfassen' eines Sachverhaltes ist, kann es seinem Wesen nach unmöglich dort sein, wo nicht in derselben Seinseinheit auch jene Erinnerungsbilder, jene übergeordneten Begriffe lebendig sind, welche jenes Wiedererkennen, jenes begriffliche Unterordnen, jenes Einordnen des gegenwärtig Erlebten in frühere Erfahrungszusammenhänge ermöglichen. Diese Seinseinheit nun ist unser Ich, ist die Persönlichkeit des Wissenden. Ein Wissen ohne einen Wissenden kann es so wenig als eine Bewegung ohne ein Bewegtes geben. In seiner Behauptung läge nicht weniger Unmöglichkeit als in dem barocken Einfall Morgensterns: „Ein Knie ging einsam durch die Welt; ein Knie, ein Knie — sonst nichts".

Und nun die Frage: Kann es ein *Erleben* geben ohne Jemanden, der erlebt? Gewiss widerspricht schon die Wortbildung 'Erleben' einer Ausserachtlassung des Erlebenden. Aber wir wollen uns nicht an die Wortbildung als solche halten, sondern fragen, was das Erleben erst zum 'Bewusstseinsvorgang' macht, und was, beseitigt, auch das Bewusstsein mit vernichten müsste? Mit anderen Worten: Was ist allem Bewusstsein notwendig immanent? Oder noch anders: Was geschieht, rein seinsmässig betrachtet, dort, wo wir von einem Bewusstseinsvorgang (hier: von einem Erlebnis) sprechen? Nichts anderes, als dass eine neue Gegebenheit (ein Ton, ein Geschmack, eine Helligkeit) eingefügt wird einem schon bestehenden, unfassenderen Seinskomplex. 'Komplex'? — nein, einer Einheit, einer umfassenden Seinseinheit, — die mit dem in sie eingehenden neuen Bestandteil nicht identisch ist, die schon bestand, bevor der neue Eindruck kam, und ohne die der Ausdruck 'Bewusstsein' inhaltslos wird. Nämlich inhaltslos wird, wenn niemand da ist, dem der Einzeleindruck bewusst wird. Das Subjekt des 'bewusst Werdens', 'bewusst Seins' ist also notwendig immer ein Jemand, dem etwas bewusst wird. Und zwar nicht allein dort, wo man unter Bewusstsein Wissen versteht, sondern auch dort, wo man darunter Erleben meint. Nur wo eine den Einzeleindruck aufnehmende Ganzheit, zu der eine Vielheit solcher von ihr zusammengehaltener Eindrücke gehört, besteht, kann Bewusstsein sein. So kann schon eine in der Zeit andauernde Empfindung nicht ohne einen Träger gedacht werden, der von der Empfindung, als einem in der Zeit ablaufenden und also jeweils im 'jetzt' sich erschöpfenden Vorgang, dadurch unterschieden ist,

dass er 'Derselbe' in den sich folgenden Momenten bleibt. Von einem Vorgang aber und also auch von der Empfindung, kann man nur sagen, dass sie in der Aufeinanderfolge der Zeit „gleich bleibt" oder 'sich ändert'. Wobei das 'sich' schon fast zuviel behauptet. Denn die Empfindung ändert sich nicht, wie sich z.B. ein Stück Wachs hinsichtlich seiner Zustände und Eigenschaften ändern kann: indem nämlich das identische Stück Materie bald diese, bald jene Gestalt, Grösse, Farbe usw. annehmen kann; als Materie dabei dauernd *dasselbe* Stückchen bleibend, welches von einem anderen Stückchen Wachs numerisch unterschieden ist (es ist 'dieses' und nicht 'jenes') selbst wenn es ihm noch so ähnlich oder gar „ganz gleich" ist. Von eben diesem selbigen, materiellen Stückchen kann man sagen, dass „es sich ändert". Nicht in gleicher Weise von der Empfindung, weil es sie als „ein in der Zeitfolge sich änderndes Identisches", überhaupt gar nicht gibt. Das ihr zugrundeliegende Identische ist: der Empfindende. Er ist es, der empfindet, und Verschiedenes empfindet, wenn ihm bald rot, bald grün geboten wird. Er ändert sich in seinem Erleben, — nur diesen Sinn kann es haben, wenn gesagt wird, 'seine Erlebnisse ändern sich'. Schon in diesem Sinne setzt also das in der Zeit sich dehnende Empfindungserleben einen Erlebenden voraus. Damit aber wäre ein *Bewusstseinserlebnis* noch keineswegs gesichert: Erst durch seine Abhebung von den vorangehenden oder gleichzeitigen Erlebnissen, also durch seine Eigenart innerhalb des in der Psyche des Erlebenden sich aufbauenden Systems von Einzelerlebnissen tritt das individuelle Erlebnis in den Lichtkreis des Bewusstseins. Ein Ich, dem zeitlebens nichts geboten wäre, als nur eine einzige andauernde Empfindung, könnte auch diese ihm zugedachte Empfindung nicht bewusst erleben. Ihm würde es so gehen wie dem Erblindeten, der das Erlebnis des Augenschwarz nur solange behält, als ihm wenigstens die Erinnerungsbilder der anderen Farben lebendig bleiben; und der mit Zurücktreten dieser Erinnerungen auch die, dem auch nicht lichtgereizten Auge eigene, Schwarzempfindung verliert. Erst durch Abhebung vom Andersartigen innerhalb derselben psychischen Einheit, desselben Ich, gewinnen die Erlebnisse Bewusstheit. Erst als einheitlicher Träger einer Mannigfaltigkeit von Einzelnem wird das Ich zum eigentlichen Bewusstseinsträger, — denn 'sich bewusst sein eines Etwas' bedeutet stets das 'Vor-sich-Haben', das

'Zum-Objekt-Haben' eines Etwas, trägt also immer das Subjekt-Objekt-Verhältnis in sich. Und dies nicht nur dort, wo es sich um Denk- oder Vorstellungsobjekte handelt und das Subjekt-Objekt-Verhältnis in die Augen springt, sondern auch schon bei jeder bewussten Empfindung, welche, mag sie das Subjekt auch noch so ergreifen und erfüllen, dennoch nie voll identisch sein kann mit dem Subjekt, — da sie stets nur einen (wenn im Moment auch einen noch so wichtigen und allem anderen überlegenen) Bestandteil seines Bewusstseins-, seines Erfahrungs-, seines Erinnerungsschatzes bildet. — Natürlich ist hier auch die Grundlage gegeben für jenen allbekannten Einfluss, den der aus der Vergangenheit mitgebrachte oder in der Gegenwart mitgegebene Gesamtzustand des Erlebenden auf das Einzelerlebnis, z.B. die Einzelempfindung, ausübt und der die Eindeutigkeit der Beziehung Reiz-Empfindung erschüttert, ja geradezu aufhebt. Seine Bedeutung geht, wie die Untersuchungen des Innsbrucker Institutes an 'Dauerversuchen' zeigen, noch viel weiter, als sie bisher, z.B. im Gebiete des Simultan- und Sukzessivkontrastes, der Adaptation usw., schon bekannt waren. Der Begriff einer 'Einzelempfindung' muss, wie dies von verschiedenen Seiten wiederholt vorgeschlagen wurde, überhaupt gestrichen werden: Ihre Eigenart erhält jede Empfindung erst durch den Hintergrund des Gesamtsystemes, auf das der Reiz einwirkt: sie ist nichts anderes als dessen Antwort auf den Reiz. Und ihre Bewusstheit ist ihre Abhebung als eines besonderen Einzelnen innerhalb der von der Ich-Einheit getragenen Vielheit.

Alles, was wir bisher ausführten, bezieht sich auf die empfangende Fähigkeit des Ich, auf sein *Beeinflusstwerdenkönnen,* so durch Aussenweltreize wie durch logische Sachverhalte. Aber das Ich könnte nicht sein, wenn es nicht neben seinem passiven, empfangenden Vermögen die *Aktivität* besässe, welche der eigentliche Seinskern von allem Seienden sein muss. Denn 'sich selbst im Sein erhalten', d.h. 'sein', bedarf der Kraft der Eigenständigkeit; alles, was Anspruch darauf machen kann, eine gewisse Selbständigkeit im Sein zu besitzen, hat sie nur, sofern sie eine eigene Aktivität besitzt, — beide Begriffe sind identisch. Im Lebewesen bilden die Aktivität sein Wille und die Triebe. Und das tiefste Leben spielt sich in deren Bereich ab. Dabei verlieren beide oben geschilderten Bewusstseinsarten ihren Sinn. Gewiss können wir unser Wollen zu unserem

Betrachtungs- und Wissensobjekt machen, aber ebenso gewiss ist es, dass dessen eigentliches Sein nicht im Betrachtetwerden besteht. Auch nicht im Erleben, sofern uns das Erleben stets etwas bietet als Objekt, das wir als solches auffassen (mag es ein erlebter Geruch, Geschmack, Ton oder Farbe sein), das 'nicht schlechterdings wir sind' — wenn es uns im Erleben auch aufs innigste berühren und durchdringen mag. Demgegenüber unsererseits immer noch ein Verhalten möglich ist, ein Stellungnehmen, ein Sichzuwenden oder Abwenden, ein Verarbeiten, ein Sichhingeben oder Sperren; beim Empfindungsschmerz: ein Unterliegen den von ihm ausgehenden Triebtendenzen oder ein Sieg unseres zentralen Willens. *Dieser Wille selbst aber braucht, um zu sein, von uns nicht nochmals zum Objekt gemacht und aufgefasst zu werden.* Er selbst ist unser Zentrum, unser aktives Ich! Daher kann er nicht erlebt werden wie eine Empfindung, als etwas in uns Auftretendes, uns Gebotenes. Wir 'erleben' ihn nicht, sondern 'wir sind er'. Daher kann auch seine Bewusstheit nicht, wie bei der Empfindung, die eines noch so innig erlebten Uns-Gegebenen, Ueber-uns-Kommenden und in diesem Sinne immer noch einer Objektgegebenheit sein, sondern muss 'in sich selbst bestehen': Der Wille lebt, indem er ist, und sein Bewusstsein ist eben dieses Leben selbst, — man könnte es vielleicht am treffendsten mit 'Selbstbewusstsein' bezeichnen, dessen Wesen darin besteht, dass hier die Beziehung von Subjekt und Objekt, welche die Grundlage aller anderen Bewusstseinsarten ist, zu sein aufhört und nur das Sein, sofern es eben ist, 'sich selbst erlebt'. Daher kann das hier Gelebte, das Willensleben, das in eben beschriebener Art als unbedingt 'bewusst', ja als der Ort unseres tiefsten, unseres eigentlichsten Seins bezeichnet werden muss, vom Standpunkt des Bewusstseins = Wissen — 'unbewusst' sein, und selbst noch dem Bewusstsein des Erlebens, mit seiner Subjekt-Objekt-Beziehung völlig fern bleiben. Und dieses umso eher, je zentraler die Willensrichtung ist, um die es geht: Denn Einzel-Willensakte können leicht zur Betrachtung und Konstatierung gezogen werden, wie sie in den Worten *„Ich will* dies oder jenes" ihren Ausdruck findet. Aber unser eigener Grundwille, der unser aktives Wesen ausmacht, wird nur selten und nur unter Ueberwindung von Schwierigkeiten Betrachtungsgegenstand. Denn damit er dies wird, darf er nicht mehr mit unserem Ich schlechthin identisch sein. Um ihn be-

trachten zu können, müssen wir von ihm abrücken, denn das Betrachtete kann nie mit dem Betrachter zusammenfallen. Unserer Grundstimmung, sei es ständige Gehobenheit oder Niedergeschlagenheit, können wir, obwohl sie uns erfüllt und geradezu 'uns ausmacht', nur bewusst werden, wenn wir bei anderen Menschen ein davon verschiedenes Lebensgefühl kennen lernen, — nun kann uns unsere Eigenart auffallen, sie kann uns zum Betrachtungs- und Konstatierungsobjekt werden, — erst jetzt erreichen wir das 'Wissen, wie wir sind'.

Aber auch schon vorher war unser Lebensgefühl tatsächlich so, wie es eben war, — der eine von uns war euphorisch, der andere depressiv, obwohl er es „nicht wusste", dass er es war. So kann man tief unglücklich sein, ohne es zu wissen, — es sei denn, dass man sich mit Hilfe des Gedächtnisses, als ein Subjekt von umfassender Erfahrung, seiner früheren Zustände erinnert, als man anders fühlte, und durch den Unterschied sich seines gegenwärtigen Zustandes bewusst wird. Für das eigentliche Lebensgefühl aber ist diese Konstatierung völlig irrelevant: es besteht ganz unabhängig davon, ob es ausserdem auch noch 'konstatiert wird', und zwar besteht es als ein höchst Lebendiges in uns, als unser eigentliches Leben, — d.h. als unser 'Bewusst-Sein'. Wobei es auf der Hand liegt, dass der Sinn dieses Bewusstseins von den beiden oben besprochenen Bewusstseinsarten verschieden ist.

Ob auch durchaus verschieden? Oder werden die drei Bewusstseinsarten durch den übergeordneten Begriff des Bewusstseins doch zurecht miteinander verbunden? Auch der dritten Bewusstseinsart, dem Selbstbewusstsein des Wollens, ist jener Grundzug des Bewusstseins eigen, dass es sich nur innerhalb übergeordneter, durch eine Seinseinheit zusammengehaltener Systeme, d.h. innerhalb von Lebewesen einstellt, oder wenigstens hier erst jene Ausgeprägtheit erreicht, die uns erlaubt, sein Vorhandensein nachzuweisen. Und dass es umso höhere Grade erreicht, je reicher das Gesamtsystem ist, in dem es auftritt, je mannigfaltiger es sich gliedert, eine je hierarchisch höher aufgebaute Einheit es darstellt. Nur innerhalb einer Persönlichkeit oder doch eines Lebewesens, allwo der Grundtrieb sich, je nach Lebensumständen, in mannigfach verschiedene Strebensrichtungen verzweigt, die selbst sich wieder tausendfältig in konkreten Einzelwillensakten und endlich Handlungen verwirk-

lichen, — ist die Seinsgrundlage gegeben für das Bewusstsein, für das *Selbstbewusstsein der Aktivität des Seins, wie es dem Grundtrieb eines Lebewesens mit seinem ganzen Ausläufersystem eigentümlich ist.* Ein unentwegt eindeutig ausgerichteter Trieb, wie er z.B. den zur Erde gravitierenden Körpern eigen ist, steigt, so gewaltig er auch immer sein mag, nicht bis zur Bewusstseinsschwelle, die sich mit der unsrigen vergleichen liesse, heran. Hier liegt die Grenze, welche wir zwischen dem bewusstseinsbegabten, belebten Sein und der 'toten Materie' zu ziehen gewohnt sind. — So sind denn alle drei Bewusstseinsbegriffe zusammengehalten durch die Beziehung des einzelnen Bestandteiles zum Seinsganzen, innerhalb dessen er sein Sein und damit auch sein Bewusstsein erhält.

Doch greifen wir von dem so erreichten Standpunkt auf die eingangs aufgestellte Transzendenz (Transmanenz) des Denkens und das sich daraus ergebende Problem der Wahrheitsfindung zurück. Dazu betrachten wir nochmals die gegenseitige Beziehung des Denkens und Erlebens auf deren Unterschied und Uebereinstimmung hin. Richtig ist, dass sich an das konkrete Erleben oft und lebhafter als an das blosse Denken Gefühle anschliessen; sodass man nicht selten von 'erleben' und 'Erlebnis' überhaupt erst spricht, wenn dabei ausgesprochene Gefühle mitbeteiligt sind, — wodurch dann auch ein noch so abstrakter Gedanke, welcher Neues, Wichtiges, Gefühlsbetontes bringt, zum 'Erlebnis' werden kann. Wir wollen uns an diesen Sprachgebrauch nicht halten, weil er, wie dieses Beispiel zeigt, das Gebiet des Denkens nicht von dem des sinnlichen Empfindens unterscheiden lässt; sondern unter Erleben den Zustand verstehen, in dem wir konkreten, anschauungserfüllten Eindrücken, wie sie uns z.B. unsere Sinne bieten, hingegeben sind. Auch diese treten, wie wir hörten, nie 'einzeln' und 'für sich' auf, sondern heben sich in ihrer Eigenart von dem gesamten Bewusstseinshintergrund ab und erlangen erst in ihrer Beziehung zu den übrigen gleichzeitigen oder vorangehenden und in unserem Gedächtnis aufgespeicherten Erfahrungen diese ihre Eigenart. Auch wenn wir keine eigentlichen Urteile dabei fällen, tragen unsere Eindrücke dennoch stets den Charakter ihrer Beziehung zur vorangehenden Erfahrung an sich: Wir können z.B. keinen Eindruck erleben, ohne dass er uns als ein 'gewohnter' oder als ein 'in seiner Neuheit

uns überraschender' Eindruck entgegentritt. Selbst seine Intensität und Qualität sind in unserem Erleben ein Ausdruck der Beziehung, welche zwischen ihm und den übrigen Erfahrungseindrücken herrscht. Darin wird man aber mit Recht auch schon die ersten Stufen des Denkens wiedererkennen. Denn was ist Denken anderes als die Schau einer Vielheit in der Einheit ihrer gegenseitigen Beziehungen? Mag diese Ueberschau auch mehr konkret anschaulich oder abstrakt begrifflich geschehen; dieser Unterschied ergibt verschiedene Typen des Denkens, wobei die Abstraktion mit ihrer Beiseitelassung des Nicht-unbedingt-Notwendigen die Ueberschau erleichtert. Wer aber auch ohne solche Vereinfachung den Gesamttatbestand als ein in sich zusammenhaltendes Bezugssystem erfassen kann, denkt und erkennt nicht weniger als der abstrakte Denker, indem er es, um einen Ausdruck Goethes zu gebrauchen, weniger als Forscher und mehr als Schauer (zu denen Goethe auch sich selbst zählte) tut. Diese Fähigkeit ist so gut wie die der Abstraktion dem einen mehr, dem anderen weniger gegeben, aber keiner von uns ist ihrer ganz ledig: Denn schon bei der Betrachtung einer beliebigen Figur, die wir als Ganzes überschauen können, erkennen wir rein anschaulich ein System von gegenseitigen Beziehungen. Sehen wir z.B. einen Keil, so heisst das, dass wir die kontinuierlich verlaufende Aenderung des Abstandes der gegenüberliegenden Keilränder.... wie soll man es nur ausdrücken: erleben oder denkend auffassen? Beides ist richtig, denn das Erleben von Systemen hat schon den wesentlichen Zug des Denkens, sofern das Einzelne nicht für sich, sondern als Bestandteil übergeordneter Systeme und damit also als Beziehungsglied eines ganzen Beziehungsnetzes aufgefasst wird. Noch primitiver ist das Erleben eines aufheulenden Sirenentones; aber auch hier gehört schon die noch 'urteilslose' Auffassung allmählicher Intensivierung des Eindruckes zum Wesen der Wahrnehmung, die ein ganzes Intensitätssystem umfasst. Und so ist also auch schon jedes Erleben, sofern es Einzelnes nie nur ganz für sich, sondern stets im Zusammenhang mit anderem und also alles in gegenseitigen Beziehungen erfasst, sobald es nur jene Differenziertheit und damit Klarheit und Deutlichkeit der inneren Struktur erreicht, die das natürliche Erleben eines erwachsenen Menschen auszeichnet, vom Denken nicht mehr grundverschie-

den. Und man beachte wohl: Seine primitiveren Schichten führen nicht etwa zur völligen Vereinzelung der Eindrücke zurück, sondern zum undifferenziert Chaotischen, Dumpfen, Dunkel- oder Unbewussten, welches nur ein Gesamterleben ohne daraus sich individuell abzeichnende Teilbestände kennt.

Vom Standpunkt einer atomistischen Auffassung des Psychischen könnte also schon beim 'Gesamtauffassen von vielem Einzelnen und dessen gegenseitigen Bezügen' der Tatbestand der Transzendenz vermutet und daher die Wahrheitsfrage gestellt werden: Werden die zwischen den einzelnen Bestandstücken bestehenden Beziehungen dabei auch 'richtig aufgefasst'? Ist doch nach dieser Auffassung nur das Einzelne das unmittelbar in seiner Eigenart Erlebte, — wo das Problem von wahr und falsch schlechthin sinnlos wird, da jedes Erlebte eben schlechthin das und so ist, wie es erlebt wird. Nun aber sind die zwischen dem Einzelnen bestehenden Beziehungen durch die Eigenart dieses Einzelnen schon eindeutig determiniert, — daher könnte deren 'Auffassung' an dem wahren Tatbestand auch schon vorbeigreifen, und also wären wir (wenn jenes skeptische Bedenken, welches alle Wahrheitsfindung aus dem Tatbestand der Transzendenz anzweifelt, zu Recht besteht) nie in der Lage, ein Beziehungssystem uns unmittelbar gegebener Elemente (wie es z.B. jede Gestalt bietet) unzweifelhaft so aufzufassen, wie es 'tatsächlich ist'! — Gewiss wird man dieses Bedenken hier nicht ernst fassen, indem man darauf erwidert, dass jene atomistische Betrachtungsweise, welche davon ausgeht, dass uns nur die Elemente unmittelbar gegeben seien, von vornherein unrichtig und undurchdenkbar ist. Nicht das einzelne Element, noch eine Vielheit von Elementen sei uns in voller Unabhängigkeit voneinander gegeben, sondern die ganze Gestalt, das ganze Beziehungssystem des Einzelnen. Darin äussere sich ja die übergeordnete Seinseinheit des Ich, dass sein Bewusstsein gleichzeitig ganze Systeme umfasst, und zwar nicht erst im abstrakten Denken, sondern auch schon im Erleben! Denn im Wahrnehmen erleben wir Farben- und Gestaltsysteme, Tonsysteme, Tastsysteme und nicht nur jedes dieser Systeme für sich, sondern auch deren Zusammenfassung, — so in der Oper die Zusammenfassung des Visuellen und Akustischen, im Tanz des Akustischen und des Bewegungssystems. Und so immer und überall in unserem Leben und Erleben, — denn unser Ich ist jenes Zentrum, welches sieht

und hört und tastet, und auch fühlt, frohlockt und leidet, und auch wünscht und will und sich entscheidet. Und das Bewusstwerden im Ich ist, wie wir hörten, nichts anderes als das Eingefügtsein des Einzeleindruckes in dieses Bewusstseinssystem des Ich: in dem das Ich alles Einzelne überblickt, von ihm 'Bewusstsein hat', — *wobei das Dasein alles Einzelnen nichts anderes ist als dessen Aufscheinen im Bewusstseinsfeld des Ich.*

Nun aber weiter: Die Aussenwelt ist uns stets im Bilde unserer Erlebnisse gegeben. Wenn wir in sie hineinschauen, so nur mit unserem Auge, welches uns die Farberlebnisse und nicht das 'Für-sich-Sein' der Dinge unmittelbar vermittelt. Horchen wir in die Natur hinein, so fassen wir, Dank unserem Ohr, das 'Erlebnisreich der Töne' auf. Im Bilde unserer Erlebnisse und also nur mittelbar erfasst steht die Natur vor uns. Wie aber steht es mit unserem eigenen Erleben, wenn wir in ihm nicht schlechthin aufgehen, sondern es auch noch 'betrachten'? Geschieht dieses Betrachten auch wieder nur 'im Bild'? Wenn ich also z.B. meine Aufmerksamkeit auf eine Druckempfindung an der Oberfläche meines Körpers richte, 'sie betrachte' und dann ihre Eigenart feststelle: als stark, als intermittierend, als ausgedehnt, als unangenehm usw.: Worin besteht eigentlich das Wesen dieser 'Betrachtung' und wodurch ist sie unterschieden von dem 'blossen Erleben' jener Druckempfindung? Dass ein solches Betrachten schon ein Denkprozess ist, kann nicht bezweifelt werden, und dennoch ist es gleichzeitig auch: ein Erleben! Worin geht denn das Betrachten über das Erleben hinaus? Entsteht etwa vor dem Auge der Betrachtung neben dem bewussten Druckerlebnis noch ein Bild davon, 'ein Bild, durch welches oder in welchem es betrechtet wird'? Mit nichten; eines solchen Betrachtungsbildes bedarf es nicht, denn das 'bewusste Erleben' ist ja selbst schon eine Art Betrachtung! Haben wir doch das Bewusstsein, wie es beim Erlebnis vorliegt, in nichts anderem als in einem 'unmittelbaren Wissen des Ich' von Tönen, Farben, Drücken usw. vorgefunden; einem durchaus anschaulichen konkreten, sinnlich angefüllten, erlebnisvollen, und nicht nur einem in fadenscheinigen abstrakten Begriffen, von denen die Lebensfülle konkreten Erlebens abgestreift ist, einherschreitenden Wissen, — aber doch in einer Art 'Erlebniswissen' von Farben, Tönen, Drücken usw. Und was ist es dann, wodurch sich die 'Betrachtung einer Empfindung' von dem

'blossen Erleben' ihrer unterscheidet? In nichts anderem als einer Gewichtsverlagerung, die allerdings so bedeutungsvoll ist, dass sie die den beiden Prozessen zugrundeliegende Gleichheit leicht übersehen lässt: Auch schon im Erleben, hörten wir, wird alles Einzelne nur als Bestandteil übergeordneter Erfahrungssysteme aufgefasst. Und doch liegt der Schwerpunkt des *Erlebens* nicht auf dem Einordnungssystem und nicht auf dem Prozess der Einordnung, sondern durchaus auf dem erlebten Inhalt selbst. Die gesättigt aufleuchtende Farbe, der intensive Ton, der angenehme oder widerliche Geruch strömen auf uns ein, erfüllen uns, lassen uns ihnen hingegeben sein. Das ist Erlebnis, — das nicht selten auch noch mit ausgesprochenen Gefühlen einhergeht. Es kann in einem einzigen Eindruck oder in einer ganzen Gestalt von Einzeleindrücken bestehen, wie es bei der Melodie, bei einer ganzen Symphonie der Fall ist. Immer gehen wir sozusagen auf in diesen mehr oder weniger komplizierten Erlebnissen. — Statt uns dem Eindruck nun einfach hinzugeben, können wir auch darauf ausgehen, ihn in Beziehung zu bringen zu früheren Eindrücken, ihn als ihnen ähnlich, gleich oder verschieden aufzufassen, seine Beschaffenheit in der Zeitfolge als unverändert oder wechselnd, seiner Provenienz nach ihn als Menschen- oder Vogelstimme, als Klavier- oder Geigenton zu bestimmen. In je zahlreichere Beziehungen wir den erhaltenen, erlebten Eindruck einordnen, — desto mehr verschiebt sich der Schwerpunkt unseres Erlebnisses: Nun ist nicht mehr der Ton als solcher die Hauptsache, sondern sein Eingefügtsein in die mannigfachsten, vielleicht schon sehr komplizierten Systeme. Unser Erleben beschränkt sich nicht auf den Ton, sondern breitet sich über weite Gebiete unserer Erinnerungen, unseres sonstigen Wissens aus. Und wir, die wir diese Verarbeitung vornehmen, gehen nicht mehr auf im aktuellen Eindruck, sondern tun gleichsam 'einen Schritt von ihm zurück', damit wir ihn erst 'betrachten', d.h. die vielfachen Beziehungen aktivieren können, die sich von ihm zu den vielen anderen Eindrücken und Eindruckssystemen lebendig machen lassen. Manche dieser Beziehungen stellen sich so unmittelbar und selbsttätig ein, dass sie zum Erleben mitzugehören scheinen. So erleben wir den durch Hundegebell erzeugten Ton nicht etwa zuerst 'schlechthin' und fassen ihn erst bei einer sich daran anschliessenden Weiterverarbeitung 'als Ton' und 'als Hundebellen'

auf, sondern er tritt uns sofort schon in dieser Einordnung entgegen. Da kann der Schwerpunkt, trotz seiner Einfügung in weitere Zusammenhänge, immer noch auf dem Erleben als solchem liegen bleiben. Je weiterreichend aber diese Verbindungen werden, je mehr Aufmerksamkeitsenergie dazu gehört, sie ins Bewusstsein zu rufen und zur Klarheit zu bringen, — desto mehr verschiebt sich der Schwerpunkt vom Erleben auf die Verarbeitung und auf das ganze dadurch im Bewusstsein entstehende, umfassende Gesamtbild, dessen integrierender Bestandteil der erlebte Eindruck ist. Nun steht der ihn so Verarbeitende gleichsam ausserhalb des Eindrucks: das ist 'der Schritt, den er von ihm zurückmacht'. Aus dem Erlebenden ist ein Betrachtender, ein Denkender geworden.

Gewiss spielen dabei die Abstraktionsvorgänge und die Begriffe, als deren Ergebnis, eine entscheidende Rolle. Aber was ist Abstraktion anderes als das Hervortreten bestimmter Bestandteile, Eigenschaften, Merkmale eines konkreten Erlebnisses auf Kosten anderer, welchen das Interesse des Erlebenden abgewandt bleibt? Der Zauberstab der Aufmerksamkeit vermag aus einem Erlebniskomplex beliebige Teile heranzuziehen, andere zurücktreten zu lassen, seien es raum-zeitlich von anderen unterschiedene, daher gleichsam auch für sich existenzfähige Teile ('dieser Ort eines viele Orte umfassenden Bildes', 'dieser Ton einer ablaufenden Tonfolge' u. dgl.), seien es für sich allein konkret nie vorkommende Merkmale ('diese Farbe', wobei 'diese' im raum-zeitlichen Sinne nur als Hinweis dient und gemeint ist z.B. 'die Röte' unter Absehung von aller bestimmten raum-zeitlichen Bindung, als Allgemeinbegriff). Dass dieses Vermögen des Absehens und Hervorhebens beim Menschen ungeheuer stark entwickelt ist und die entsprechenden Fähigkeiten der übrigen Lebewesen unvergleichlich überragt, macht ihn zum homo sapiens. Unmöglich aber ist es, sich ein lebensfähiges Wesen vorzustellen, dem sie ganz abginge. Ein solches besässe überhaupt keine Aufmerksamkeit, alles ihm Gebotene müsste sein Bewusstsein immer gleichmässig erfüllen, was das Lebewesen vollkommen handlungsunfähig machte, denn die Handlung setzt Konzentration der Vorstellungs- und Willensenergie nach einer bestimmten Richtung voraus. Ein solches Lebewesen wäre aber auch eindeutig an den individuell konkreten Eindruck gebunden, könnte sich der nie absolut gleich-

bleibenden Erfahrung überhaupt nicht anpassen. Sobald ihm z.B. das Nahrungsmittel in anderer Menge oder Form geboten wäre, könnte es die übereinstimmenden, gemeinsamen Züge grundsätzlich nicht herausheben und wiedererkennen und sein Verhalten nicht danach richten. So setzt denn jedes Wiedererkennen unter mehr oder weniger veränderten Umständen, wie es bei jedem 'blossen Erleben' schon vorkommt, eine gewisse Fähigkeit der Abstraktion voraus. Denn wir hörten ja, das Bewusstsein, und mag es auch nur das Bewusstsein des Erlebens sein, setzt seinem Wesen nach das Einfügen des Einzeleindruckes in ein Erfahrungssystem der Erinnerung voraus. Daher erweckt in uns die Schaufel den Eindruck 'leicht', wenn wir vorher mit schwereren gearbeitet hatten, und dieselbe Schaufel den Eindruck 'schwer', wenn die vorausgehende Erfahrung uns leichte Schaufeln in die Hand gespielt hatte. Damit ein solcher Eindruck aber entstehen kann, musste sich in uns das besondere Merkmal einer 'bestimmten gewohnten Schwere' von allen anderen Eindrücken schon abgesondert und als solches konsolidiert haben: als erster Schritt zur Bildung eines Begriffes, dem die darauf folgenden Einzeleindrücke entweder untergeordnet oder zu dem sie in Gegensatz gebracht werden. Und ein solches Arbeiten mit Begriffen macht zweifellos das Wesen auch des Urteilsvorganges aus. Mit anderen Worten *ist das Denken im Phänomen des Bewusstseins geradeso schon einbeschlossen wie das Erleben.* D.h. die beiden unterscheiden sich zwar wesentlich hinsichtlich ihres Schwerpunktes, der bei dem einen im sinnlich Momentan-Gegebenen, beim anderen in dessen Einfügung in das ganze Wissenssystem liegt, — aber *es gibt kein Bewusstsein und damit auch kein Erleben, das diesen Wesenszug des Denkens* (Hervorheben bestimmter Teilbestände und ihr Einfügen in übergeordnete Systeme) *nicht auch schon an sich trüge.* Der Unterschied zwischen den beiden ist nicht ein Qualitätsunterschied von Grund auf, sondern ein quantitativer Verteilungsunterschied der dabei beteiligten Faktoren.

Aber wie stimmt die hier vertretene Ansicht zu jenen Feststellungen, von denen wir im Anfang ausgegangen waren: betreffend die Transzendenz des Denkens und die Immanenz des Erlebens? Dieser Unterschied, welcher von wichtigsten erkenntnistheoretischen Folgerungen begleitet war, schien uns damals Denken und Erleben von Grund auf zu trennen. Denn Denken

konnte Dank dieser Transzendenz wahr und falsch sein. Ja, es musste wahr oder falsch sein. Während das Empfinden, Fühlen, Erleben unmöglich falsch sein konnte; denn es war schlechthin, was es war. Und auch die Eigenschaft der Wahrheit schien darauf nicht anwendbar zu sein. — Aber was nicht falsch sein kann, braucht deswegen noch nicht zu jener Kategorie der Gegenstände zu gehören, die auch nicht wahr sein können (wie dies z.B. für die Vorgänge der Aussenwelt tatsächlich gilt, die nur sein oder nicht sein, nicht aber wahr oder falsch sein können). So sind unmittelbar einleuchtende Einsichten ('drei ist grösser als zwei') unmöglich falsch, weil sie notwendig wahr sind. Dass ein Urteil wie das obenstehende einen Denkakt ausdrückt, ist zweifellos. Aber wodurch unterscheidet es sich denn eigentlich vom blossen *Sinneneindruck* zweier Mengen, die aus zwei und drei beliebigen Gegenständen, z.B. Aepfeln, bestehen und die in uns das *unmittelbare Erlebnis* des Grösserseins der zweiten gegenüber der ersten Menge hervorrufen? Ja, man kann hier vom Grössererlebnis sprechen, denn dürfte man dies hier nicht, so dürfte man auch beim Aufheulen einer Sirene nicht sagen, dass wir die Tonverstärkung unmittelbar erlebten, — dann könnten wir überhaupt nie von erlebten Beziehungen sprechen, nicht beim Zunehmen und Abnehmen eines Schmerzes, nicht beim Nachlassen einer Uebelkeit. Dann könnten wir auch nicht die Verschiedenheit der uns gleichzeitig gebotenen bunten Farben erleben, noch irgendwelche Gestalten erlebnismässig erfassen. Was bliebe dann überhaupt noch für das Erleben übrig? Die einzelnen Elementarempfindungen? Aber die 'Elementarempfindung' ist ein Begriff, den wir auf unsere Erlebnisse überhaupt nicht anwenden können, denn nirgends finden wir im Erleben solche Elementarempfindungen vor, immer nur in Beziehungssysteme eingebaute Einzeleindrücke. Und selbst dort noch, wo scheinbar einzelne Empfindungen in uns auftreten, erweisen sie sich in ihrer Beschaffenheit abhängig von ihrer Beziehung zu ihnen vorangegangenen Erlebnissen. — Nein, *wir erleben* zweifellos *Beziehungen*, und das heisst allerdings nichts anderes, als dass sie ganz unmittelbar in unserem Bewusstsein mit sinnlicher Lebendigkeit und als konstituierende Momente von Gesamterlebnissen, d.h. nicht anders uns gegeben sind, als Töne, Farben, Gerüche, Schmerzen usw. Wenn aber dem so ist, dann ist jedes Erlebnis,

welches sich in allen seinen Elementen zur Klarheit und Deutlichkeit entwickelt, zugleich eine Erkenntnis: Klar und deutlich kann das Erlebnis jener beiden Mengeneindrücke nur sein, wenn auch ihr Unterschied uns ins Bewusstsein tritt. Und dieses kann nur geschehen, wenn uns der typische Charakter dieses Unterschiedes, also zunächst die Quantität und dann die relative Grösse dieser Quantität bewusst wird. Dabei braucht der Zahlbegriff noch nicht mit voller Klarheit schon ausgebildet zu sein; auch Tiere, welche ihn noch nicht besitzen, unterscheiden dennoch zwischen 'mehr und weniger'. Und wie wir oben die neuergriffene Schaufel das eine Mal als leicht, das andere Mal als schwer erlebten, so erleben wir auch bei den beiden Mengen von drei und zwei Objekten: hier mehr, hier weniger. Und in diesem Erlebnis steckt eigentlich auch schon dessen Verallgemeinerung: Noch mag es zunächst an den konkreten Eindruck der Aepfel gebunden sein; aber diese Bindung ist im Grunde eine nur ganz äusserliche, denn im Kern des Erlebnisses liegen doch nicht die Aepfel, sondern liegt die Auffassung der Menge. Hier ist eine grössere, dort eine kleinere Menge. Es ist dasselbe Erlebnis, das wir dann etwa bei den Kartoffeln, Bohnen oder Hunden und Pferden haben. Und wenn sich dieses ursprüngliche Erlebnis so weit reinigt, d.h. sich dessen eigentlicher Sinn mit voller Klarheit und Deutlichkeit im Bewusstsein einstellt, so gewinnt er die sprachliche Formung in der Aussage: 'drei ist grösser als zwei'.

Auch schon in jedem Erlebnis wird also eine Tatsache (ein Ton, eine Farbe, ein Schmerz) oder ein Tatbestand (ein System von Farben, Tönen usw.) in seiner Eigenart aufgefasst. Daher ist jedes Erlebnis zugleich auch eine Erkenntnis, die uns das Wesen des Tones, der Farbe oder deren gestalteter Vielheit bringt. Jedoch wird man das Wort Erkenntnis nur dort gebrauchen, wo das Erlebnis zu einer inneren Klarheit und Deutlichkeit gelangt. Und dies ist keineswegs stets der Fall; im Gegenteil, die meisten unserer Erlebnisse erreichen diese Klarheit nicht. Und es hat einen guten Sinn, vom eigentlichen Denken erst dort zu sprechen, wo diese Entwicklung sich schon soweit vollzogen hat, dass der Erlebende nicht nur einen undifferenzierten Allgemeineindruck besitzt, sondern dessen genauere und detaillierte Struktur erfasst, erlebt, erkennt. Und so führt uns die innere Verdeutlichung unseres Erlebens unmittelbar und

ohne qualitativen Sprung, hinüber in das Gebiet des Denkens. Und dieses Denken muss notwendig wahr sein, denn es ist nichts anderes als das verdeutlichte Erleben des unmittelbar Gegebenen.

Hier nun muss man streng unterscheiden zwischen dem 'unmittelbar' und dem nur 'mittelbar, im Bild Gedachten'. Das Erste setzt voraus den unmittelbaren Bewusstseins = Erkenntnisbesitz. Auf die Bezeichnung, das Wort, den Ausdruck, auf das ganze Zeichensystem der Sprache kommt es dabei noch gar nicht an; nur sofern die Bewusstseinsinhalte als solche uns unmittelbar gegenüberstehen, ist Erkenntnis da. Und eben diese unmittelbare Erkenntnis ist notwendig wahr, — mag sie sich bis zur Abstraktion und damit ins begriffliche Gebiet erheben, mag sie zunächst auch nur am konkreten Einzelfall gehaftet bleiben. Aber gewiss gilt die unbedingte Wahrheit nicht allein von dem konkret Erlebten, sondern auch von dem 'abstrakt Erlebten', d.h. dem mit Abstraktion von so und so vielen Erlebnisinhalten, die als unwesentlich ausseracht gelassen wurden, und mit Hervorhebung dessen, worauf die Aufmerksamkeit als auf das Interessierende und also Wesentliche sich richtete. Wir greifen wieder auf unser Beispiel der Grössenbeziehungen zurück: Aus einem einzigen oder auch aus wiederholtem Erleben unter wechselnden Nebenumständen wird der Bewusstseinsinhalt 'grösser' herausgehoben und damit also die Idee der Grösser-Beziehung gebildet. Nun bietet uns die folgende Erfahrung mehrere, deutlich verschiedene Mengen, deren gegenseitige Beziehungen wir entsprechend unserer Grösser-Idee erleben: Kann hier ein falsches Urteil sich noch einschleichen? Ja, sofern entweder unser Begriff des 'Grösser' oder unsere Auffassung der gebotenen Mengen nich zur vollen Klarheit gelangt waren; sofern diese Bedingung aber erfüllt war, kann das skeptische Argument der Transzendenz die Wahrheit unseres Denkens unmöglich treffen: Denn in diesem unmittelbar ihren Gegenstand erlebenden Denken gibt es keine Transzendenz mehr, — das seinem Wesen nach evidente, einleuchtende Urteil kennt keine Transzendenz, es nimmt unmittelbar teil am Wesen jener Ideen, die es verwendet. Der einzelne Denkakt ist zwar mit den gedachten Ideen nicht identisch, denn die Ideen sind auch schon vor ihm da und bleiben nach ihm bestehen. Aber: sofern sich die Ideen in unserem Bewusstsein offenbaren, sind sie genau das, als was

wir sie denken. Ob wir das Wort 'Röte' oder ein beliebiges anderes Sprachzeichen dafür verwenden, ist dabei ganz irrelevant: Wenn es mir nur ein einziges Mal gelungen ist, diese Farbart als solche von anderen zu unterscheiden und in ihrer Eigenart herauszusondern, so kenne ich von nun an die Idee der Röte und ein Irrtum ist weder in meinem Denken der Röte, noch auch in der Anwendung dieses Begriffes auf mir ebenfalls klar gegebene Einzelfarbeindrücke grundsätzlich möglich.

Eine gewisse Transzendenz könnte man dort zu erkennen meinen, wo mehrere Erlebnisinhalte als in bestimmter Beziehung zueinander stehend behauptet werden. Denn in der Tat, vom Standpunkt eines jeden Erlebnisinhaltes aus betrachtet wird von ihm etwas über sein Einzelsein Hinausgreifendes, sein Einzelsein Transzendierendes behauptet. So wird z.B. ein Begriff ihm übergeordnet, welcher schon von früher her bestand, — wird er auch auf diesen Fall richtig anzuwenden sein? Er wird durch diesen Begriff mit einem anderen Einzelinhalt verbunden: aber ob auch mit Recht? Wer kann es wissen? — Die Antwort ist eindeutig: Nur dasjenige Bewusstseinszentrum kann es wissen, welches seinem Sein und Wesen nach weder durch den einen noch durch den anderen Bewusstseinsinhalt begrenzt ist, dessen Erleben sie alle von einem einheitlichen Seinszentrum aus erfasst, in dem sie also alle in einer Einheit vereinigt sind, und für das die vom Standpunkt jedes Einzelinhaltes bestehende Transzendenz nicht gilt, da die jeden dieser Einzelinhalte transzendierenden Bestimmungen diesem Seinszentrum sämtlich immanent sind.

So sind denn die Urteile sämtlicher Wissenschaften, welche einleuchtende Begriffsbeziehungen und Begriffssysteme bilden (wie es z.B. die Mathematik tut) dem skeptischen Argument der Transzendenz unzugänglich. Denn überall, wo Denkevidenz herrscht, besteht eine höhere Seinseinheit, welche die Zersplitterung des Vielen nicht mitmacht, welche es vielmehr in ihrer eigenen Einheit zusammenhält. Wie aber steht es mit unseren Urteilen über die uns umgebende *Aussenwelt,* — hier scheint doch unbedingt eine Transzendenz der Seinseinheit unseres Ichs vorzuliegen und daher das skeptische Argument in voller Kraft zu sein? — Aber auch hier ist die Sachlage nach dem oben Ausgeführten klar vorbestimmt: Entweder besitzen wir überhaupt keine Erkenntnis über unsere Aussenwelt, der das

Merkmal der Evidenz zukäme, — dann greift das skeptische Argument in seine Rechte voll ein und die Entscheidung über Wahrheit oder Falschheit unserer Ansichten von der Umwelt ist schlechterdings unmöglich. Oder aber gibt es auch hier klar evidente Grundeinsichten, — dann aber kann auch hier die Transzendenz zwischen dem Sein des Denkenden und dem des von ihm Gedachten durchgehend nicht bestehen. Auch dieser Fall ist ja gewiss nicht unmöglich: Steht es denn wirklich so, dass unser Ich eine letzteigene, in ihrem Sein und ihrer Grundbeschaffenheit von allem anderen Sein vollkommen ab- und in sich eingeschlossene Substanz sei? Dann freilich wäre ein Ueberspringen des Wissens von der Welt zum Ich nicht möglich. Besitzen wir aber Grundbegriffe von solcher Allgemeinheit, dass sie sich schlechterdings auf jedes Sein beziehen, indem sie den Begriff des Seins geradezu konstituieren, — so ist die Sorge unbegründet, ja undenkbar, dass es ein Sein je geben könnte, das sich zu unserem Seinsbegriff in Widerspruch befände. Zu solchen Grundbegriffen und aus ihnen sich ergebenden Grunderkenntnissen gehört der Satz der Identität, der Satz des Widerspruches und des zureichenden Grundes, wie er sich auch im Kausalgesetze geltend macht. Ein Sein, welches wohl wäre, aber dennoch mit sich selbst nicht identisch wäre, ist schlechterdings ein Widerspruch in sich, wie ein 'rundes Dreieck', also ein Dreieck, welches zugleich kein Dreieck, sondern ein Kreis ist. Sprechen wir die Worte 'rundes Dreieck' aus, so denken wir zwar die drei Begriffe: 'Dreieckigkeit', 'Rundung' und den Begriff der 'attributiven Beziehung', welcher in dem Verhältnis zwischen dem Substantiv und dem darauf bezogenen Adjektiv liegt. Diesen Begriff denken wir aber zunächst nur allgemein, d.h. wir wissen, worin die attributive Beziehung zwischen zwei Begriffen besteht, welches Verhältnis wir also zwischen 'Drei-Eck' und 'rund' denken sollen, wenn wir die Worte 'rundes Dreieck' hören. Sobald wir aber die beiden Begriffe rund und Dreieck im attributiven Verhältnis zueinander zu denken unternehmen, sehen wir auch schon ein, dass wir damit nicht durchkommen, dass hier uns eine Aufgabe gestellt is, die ihrer Natur nach grundsätzlich nicht zu lösen ist. Das und nichts anderes besagt die *Undenkbarkeit* des Begriffes 'rundes Dreieck'. Rundes Dreieck ist ein leerer Gesamtbegriff, indem durch den einen Teilbegriff etwas gesetzt wird (Eckigkeit), was durch den

anderen Teilbegriff (Rundung = keine Eckigkeit) wieder aufgehoben wird, sodass ein zu denkender Inhalt überhaupt nicht mehr zurückbleibt. 'Rundes Dreieck' ist zwar eine Zeichenfolge, welche uns, wie jede Zeichenfolge, zum Denken auffordert, zugleich aber in ihrem Gesamtbestand schlechterdings keinen Sinn, d.h. nichts zu Denkendes enthält. — Und wenn wir etwa die Wendung gebrauchen: 'ein rundes Dreieck ist unmöglich', so bedeutet dies keineswegs, dass ein rundes Dreieck zwar denkbar und von uns gedacht, aber zugleich seins-unmöglich sei, — wie diese Worte oft verstanden werden. Sondern seine Deutung kann nur die folgende sein: Da sich bei den Worten 'rundes Dreieck' nichts denken lässt, weil sie schlechterdings keinen denkbaren Gesamtsinn beinhalten, so fällt auch die Frage nach der Seinsmöglichkeit des 'runden Dreiecks' als sinnlos dahin. Und sinnlos ist sie, da nach der Seinsmöglichkeit nur von etwas gefragt werden kann, was zunächst als solches gedacht worden ist. — Und genau dieselben Verhältnisse wie für dieses konkrete Einzelbeispiel gelten für das weltumfassende Problem der Seinsbeschaffenheit der Aussenwelt. „Gilt der Identitätssatz für das Sein der uns umgebenden Welt?" Die Frage, ob es ein dem Identitätsgesetz widersprechendes Sein gibt oder nicht gibt, kann nur sinnvoll sein, wenn es uns zunächst einmal gelungen wäre, die beiden Begriffe Sein und 'sich-selbst-nicht-identisch' miteinander zu verbinden; dann hätten wir zwei Seinsbegriffe (sich-selbst-identisches und sich-selbst-nicht-identisches Sein) und könnten fragen, welcher von ihnen nun etwa für die uns umgebende Welt gelten mag. Da uns diese Aufgabe aber ebensowenig gelingt, wie diejenige ein rundes Dreieck zu denken, indem der Identitätsbegriff dem Seinsbegriff konstitutiv eingebaut ist, so ist auch die Frage danach, ob der Identitätssatz auch für die uns umgebende Aussenwelt gilt oder nicht, schlechterdings sinnlos! Und genau ebenso, wie wir sorglos behaupten konnten: die Idee der Röte ist genau das und so, was und wie wir sie denken, denn in unserem Denken ist sie uns ganz unmittelbar als ein bestimmter Inhalt klar gegeben, so können wir mit derselben Unbesorgtheit behaupten, dass der Satz der Identität, des Widerspruches und so auch sämtliche übrigen den Begriff des Seins konstituierenden Begriffe für die 'seiende Aussenwelt', d.h. also schlechthin für jede Aussenwelt, welche nicht nur phantasiert

sein, sondern wirklich sein soll, von unbedingter Geltung sind.

Dem Denken solcher letzten Begriffe und Begriffsbeziehungen von allgemeiner Gültigkeit ist eine besondere Nuance eigentümlich: Aller uns durch bloss sinnliche Beobachtung übermittelten Gesetzmässigkeit hängt etwas von Zwang, der uns dabei angelastet wird, an: 'Sämtliche massenbegabte Körper ziehen sich gegenseitig an', 'elektrisch geladene Körper stossen sich aber bald ab, bald ziehen sie sich an, je nach der gegenseitigen Poligkeit', — gewiss, das ist alles richtig, aber 'es könnte doch auch anders sein'. Und solange wir uns auch das Gegenteil denken können, ist es ein Zwang, den uns die Erfahrung antut, indem sie uns veranlasst, von den beiden Möglichkeiten nur an die eine als die in der Natur realisierte zu denken, nur an sie zu glauben. Ganz anders dort, wo wir unmittelbar erkennen, dass Etwas seinem Wesen nach so sein muss, wie es ist, und anders nicht sein kann. Dass, um bei unserem Beispiel zu bleiben, jedes Ding in sich selbst identisch sein muss, dass Widersprechendes nicht zu einer Einheit verknüpft werden kann, dass ein Werden nicht ursachlos auftreten kann; aber auch schon bei jedem unmittelbar einleuchtenden Satz, wie sie z.B. das Axiomsystem der euklidischen Geometrie enthält ('der gerade Weg ist von sämtlichen zwischen zwei Punkten möglichen der kürzeste' u.dgl.m.). Bei blossen Beobachtungsgesetzen sind wir angewiesen die Regel, die uns die Natur vorführt, blind zu übernehmen; bei einsichtigen Verhältnissen aber übernehmen wir nicht eine uns kalt von aussen aufgedrängte, 'nun einmal geltende' Tatsächlichkeit, sondern — sollte hier dies Wort nicht gerade den innersten Tatbestand zum Ausdruck bringen? — wir '*erleben* schlechtweg die Begriffe und ihre gegenseitigen Grundverhältnisse'. Wir betrachten sie nicht 'von aussen', sondern nehmen an ihrer innersten Beschaffenheit und daher auch an ihren Beziehungsverhältnissen unmittelbar teil, sie sind und bieten sich uns in uns so, wie sie sind. Genau so, wie sich uns die Farbe im Farberlebnis, der Schmerz im Schmerzerlebnis bietet. Zwar sind es nicht konkret sinnlich erfüllte, sondern erst nach Eintritt der Abstraktion gewordene Erlebnisse, — und will man das Wort Erlebnis nur für das sinnlich Erfüllte verwenden, so sind es keine eigentlichen Erlebnisse, die sich uns da bieten. Will man aber durch das Wort jene Innigkeit der Seinsbeziehung zwischen dem Denkenden und dem Gedachten, jene Unmittelbarkeit der

Erfassung eines Inhaltes im Bewusstsein, mag er nun abstrakt oder konkret sein, jene unmittelbare Teilnahme an ihm, jenes 'schlechthinnige' Bewusstwerden eines Inhaltes in uns bezeichnen, wie es dem 'Erleben' eigen ist, so erscheint die Anwendung dieses Wortes berechtigt — und aufklärend zugleich, denn es hebt den entscheidenden Grundzug des *'einsichtigen Denkens'* hervor: In ihm *erleben* wir die letzten Seinsbestimmtheiten, daher kann es seinem Wesen nach, als aller Transzendenz entledigt, unmöglich falsch sein und muss, wie alles Erlebte, notwendig so sein, wie es erlebt wird.

Aber greifen wir nochmals auf das Problem der Transzendenz der *Aussenwelt* zurück, wie wir es im Anfang unseres Aufsatzes formuliert hatten. Es scheint durch diese Betrachtungen noch nicht erschöpft zu sein: Mag auch die Aussenwelt, wenn sie schon einmal ist, nicht anders sein können, als jene Grundbegriffe sie bestimmen. Dass sie aber überhaupt sein soll, — das scheint doch eine unser eigenes Bewusstsein ganz eklatant transzendierende Behauptung zu sein. Und da die 'Aussenwelt' als solche, ihrer Definition gemäss, nicht in uns eintreten und in uns sein kann, so scheint alles, was wir von ihr behaupten, notwendig unser Eigensein zu transzendieren und daher dem skeptischen Zweifelsargument ausgesetzt bleiben zu müssen. — Aber wenn die Aussenwelt 'nie in uns eingehen kann, ohne ihr Wesen als Aussenwelt zu verlieren', so fragt es sich: Wie kommen wir denn überhaupt zu einem Begriff der Aussenwelt? — Dies wäre auch in der Tat unmöglich, wenn unser Ich jene seinsselbständige und daher in sich vollkommen abgekapselte Substanz sein würde, als die es oft bedenkenlos angesehen wird. — Ja, wenn die Idee der Aussenwelt eine 'rein negative Idee' wäre, — schlechthin: das 'Nicht-Ich', — so könnte sie im Ich vielleicht durch blosse Negation auch entstanden sein. Aber eine bloss negative Idee (wenn es 'bloss negative Ideen' überhaupt gibt!) ist die Idee der Aussenwelt gewiss nicht. Wird der Aussenwelt doch Sein zugesprochen, sie ist das über den Grenzen meines Ich liegende Sein, und Sein ist stets eminent positiv, ist der Träger aller Position. — Man könnte vielleicht meinen, durch 'Fallenlassen der Beschränkung' ans Ziel zu kommen: Indem wir selber sind, erleben wir das Sein; zunächst das eigene Sein, dann aber, indem wir die Beschränkung auf das Ich fallen lassen, träte uns das Sein in seiner Unbeschränktheit entgegen. Damit wir

aber mit dem Fallenlassen der Beschränkung nicht auch das Sein verlieren (Nicht-Ich = Nichts), müssen wir die beiden auseinanderhalten können. Und dieses ist nur möglich, wenn wir den Begriff des Ich besitzen, der selbst wiederum nur dort bestehen kann, wo Ich und Umwelt einander als Komplementärbegriffe schon gegenüberstehen, wo uns das 'Ich' als ein Bestandteil des, Ich und Aussenwelt umfassenden, Gesamtseins entgegentritt.

Und in der Tat besitzen wir ein solches Wissen von dem unser Ich überschreitenden Gesamtsein. Denn unseres Ich sind wir uns doch gewiss bewusst; und *ist* das Ich nicht eine letzt selbständige, von allem Aussensein vollkommen unabhängige Substanz, sondern seinem Grundwesen nach nur ein integrierender Bestandteil der Welt, — so muss ihm diese Tatsache selbst eingeschrieben sein. D.h. die Selbsterkenntnis, die uns das Ich so zeigt, wie es ist, muss, da Ich wesentlich 'Bestandteil' ist, es uns notwendig als Bestandteil auch aufweisen! Indem wir das Ich erleben, muss unser Icherleben schon jene Züge an sich tragen, die dem Ich als dem Bestandteil einer höheren Seinseinheit unweigerlich eigen sein müssen! Und in der Tat, es ist auch so. Denn vom ersten Atemzug an erleben wir uns als 'abhängig' von etwas, das wir nicht sind, als 'bedürftig' eines Etwas, das wir nicht haben. Unsere Instinkte, die allein uns lebensfähig machen, sind die Erlebnis-Brücke zwischen uns und Welt. In unserem Begehren nach Luft, nach Nahrung vom ersten Anbeginn unseres Seins an liegt schon implizit die Idee einer Umwelt, eines unser Eigensein übersteigenden Gesamtseins, von dem wir uns Hilfte erwarten, enthalten. Unser ganzer Organismus fühlt seine Unselbständigkeit und Ergänzungsbedürftigkeit und unsere Instinkte sind die aktive Aeusserung von diesem Eigenmangel und unserem Streben nach dessen Befriedigung aus einem Sein, das jenseits unseres Eigendaseins liegt. Indem wir also uns selbst erleben, erleben wir gleichzeitig auch die Idee der Welt, als deren Teilsein wir uns selbst gegeben sind. Das ist nur möglich, wenn wir im Sein und Denken 'teilnehmen' an der Welt, d.h. wenn unser Sein und Denken nicht nur in unserem Eigensein begründet sind, sondern darüberhinaus auch in dem Sein der Umwelt. Das aber heisst, dass unser Ich in sich und nur in sich weder seins- noch denkmöglich ist, dass es nur vom Dasein der

Gesamtwelt sein Sondersein bezieht, und nur sofern es durch diese Beziehung *am Sein und am Erleben* der Umwelt teilnimmt, sich selbst auch als Bestandteil des Gesamtseins auffassen kann. — Es ist so, wie Spinoza sagt: Unser Erleben ist nur ein Teil des Selbsterlebens der Gesamtwelteinheit.

Dies alles liegt schon im Urbezugserlebnis zwischen Lebewesen und Umwelt einbeschlossen. Unser abstrahierendes Denken tut nichts anderes, als dass es diese erlebten Seinsbezüge zur Klarheit emporhebt. Und auch das allbestimmende Kausalprinzip, wie es unserem Denken zugrundeliegt, ist selbst nichts anderes als das klargestellte, erlebte Grundverhältnis jedes Einzelseins zu allem übrigen Weltsein. Wir können nicht unser Bewusstsein auf unser Ich beschränken, denn das Kausalprinzip verlangt, dass alles Neuauftreten und alle Aenderung nicht schlechtweg aus sich heraus entstehen können, sondern einer es bedingenden Ursache notwendig bedürfen. In unserem Ich aber geschieht unendlich viel, was aus ihm selbst kausal nicht abzuleiten ist. Eine jede Sinnesempfindung ist dafür ein Beispiel. Daher muss die begründende Ursache jenseits seines engeren Seinsbezirkes liegen. Und die Gesamtheit jenes das Ich-Geschehen begründenden umfassenden Geschehens ist uns das Walten der Natur um uns.

So fassen wir denn das Ergebnis kurz zusammen: Nur durch Auflösung der Seinstranszendenz von Denken und Gedachtem kann echte Erkenntnis, wie wir sie im 'nur wahr sein könnenden Erleben' besitzen, werden. Ueberall wo Denken zur Erkenntnis, zur Evidenz, zur Wahrheit vordringt, muss es den Transzendenzstrich übersteigen, muss es grundsätzlich zum, sei's anschaulichen, sei's abstrakten, *Erleben des Gedachten* werden. Und erst indem das Denken zum Erlebnis wird, kann es, zum Trotz dem skeptischen Bedenken, seiner Wahrheit untrüglich sicher sein.

LE FILM COMME METHODE PROJECTIVE

PAR

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La présente communication a pour but de montrer que le film peut être utilisé comme un procédé d'analyse projective plus fécond peut-être que d'autres moyens utilisés, tels que le test de Rorschach, le T.A.T. (Thématic Apperception Test), le test de Szondi, le psychodrame de Moyeno, etc.

Voici d'abord une constatation initiale, qui est nécessaire à la compréhension de ce qui suivra.

Les psychologues se sont beaucoup intéressés, depuis dix ans à l'étude du film; il est facile néanmoins de remarquer qu'ils ont surtout consacré leur attention à étudier l'image filmée, éventuellement en la comparant avec d'autres images, ou bien à rechercher quels sont les facteurs de l'impression de réalité qu'offre le film, ou encore à déterminer dans un but pratique: ce qui conditionne la perception de la profondeur, celle de la couleur, etc.

Il me semble que dans l'étude du film c'est l'examen de spectateur qui est le plus intéressant. Depuis déjà de longues années je m'occupe de ce problème [1]). Je suis ainsi arrivé à des conclusions, dont je rappelle ici, sommairement quelques-unes.

Lorsque le film nous présente un événement, il laisse aux spectateurs la liberté de l'interpréter, de «le comprendre», d'en pénétrer non seulement le sens mais aussi la valeur. Le cinéaste n'est pas là pour nous dire ce qui arrive dans le récit filmé ni pour nous le commenter; la différence entre les premiers films sonores et ceux d'aujourd'hui (je veux dire ceux qui ont une

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[1]) De mes divers écrits sur ce sujet, je rappele seulement: «*Intérêt des projections cinématographiques*», paru il y a déjà longtemps, en 1928, au «Journal de Psychologie normale et pathologique», dans lequel je montrais l'intérêt de l'étude des directions subjectives et de leurs variations.

valeur psychologique) consiste en ceci que dans les premiers l'élément sonore nous indiquait ce que nous devions voir; le cinéaste était présent et disait au spectateur, éventuellement à travers les dialogues des acteurs, ce qu'il devait voir; dans beaucoup de cas, la sonorisation disait tout ce que le spectateur voyait. Dans les films sonores d'aujourd'hui on note une grande sobriété; il y a même des scènes pendant lesquelles il n'y a pas trace de dialogues; quelquefois un simple bruit accompagne les images (le tic-tac d'une horloge, une fontaine qui coule, une porte qui claque, etc.). Le cinéaste moderne a appris l'art psychologique subtil de nous faire pénétrer le sens et l'évolution des événements à travers des riens. Et ainsi le film moderne laisse au spectateur la liberté d'interprétation.

Mais cette liberté laissée au spectateur par le film est si grande que parfois elle lui rend ambiguë l'interprétation du sens; et cette liberté dans l'interprétation de la succession des événements, grâce à l'ambiguïté, coopère à tenir l'intérêt éveillé. Le spectateur se trouve contraint de jouer à participer à l'action du film, de s'y introduire, de la vivre, exactement comme dans la vie on vit les événements auxquels on se trouve réellement mêlé.

Le spectateur ne reste pas passif comme lorsqu'il assiste à des projections fixes successives; il en arrive à oublier que ce qu'il voit sur l'écran est un film. Il s'aperçoit si peu de la différence de la réalité filmée avec la réalité quotidienne qu'il n'est pas besoin que la projection lui donne la sensation de la profondeur; et de même, il n'est pas nécessaire que les hommes, les choses, le décor aient leurs couleurs.

Dans le film, il est important de présenter au spectateur une action dans laquelle il puisse s'engager, de telle sorte qu'il puisse vivre cette action comme il vit les actions de la vie réelle c'est-à-dire présentes, et la percevoir en état de tension, d'incertitude, de souffrance et de joie, comme pour toute action humaine.

Les personnages ne sont pas présentés au spectateur comme si chacun d'eux avait une étiquette portant son nom. Le spectateur doit comprendre qui ils sont; il doit reconnaître comme un tel ou un tel ayant tel caractère, et poursuivant tel but; il doit pénétrer dans la vie de chacun d'eux, se rendre compte du sens de leurs actions; il doit voir même ce qui n'est pas projeté sur l'écran, vivre avec eux leur passé et se projeter avec eux

sur l'avenir, sur cet avenir qui lui est indiqué par un geste, une situation, ou un objet.

C'est donc un caractère de la projection cinématographique que de laisser au spectateur sa pleine liberté, qui lui permet d'interpréter l'action présente et de souffrir (ou de se réjouir) pour ce qu'il sait qui doit arriver sous peu et qu'il s'attend à voir réalisé dans les projections suivantes. Cette liberté ne doit pas être illimitée; il suffit qu'elle laisse au spectateur l'ambiguïté de l'interprétation.

Le spectateur arrive à une interprétation du film, surtout parce qu'il est amené à projeter hors de lui-même dans un personnage, dans un milieu déterminé, dans une action ses propres sentiments, ses tendances, ses désirs, en un mot son moi, et tout spécialement son inconscient profond.

En un mot, pendant la projection du film c'est le spectateur qui vit et agit: la valeur d'un film réside précisément dans la réalisation des conditions nécessaires pour que cela se produise. Si cela vient à manquer, il n'y a plus d'intérêt. Ces considérations permettent d'affirmer que le film dépasse l'image.

Autre observation: dans la projection du film sont excités divers organes des sens. Ce fait a été mis nettement en lumière par M. le Prof. Ponzo au Congrès de Filmologie de Venise. Mais cela ne veut pas dire que toutes ces excitations sensorielles, principalement auditives et visuelles, sont toujours parfaitement synchronisées. Tandis que l'acteur agit dans un certain sens, les stimulations sensorielles du milieu peuvent agir dans un autre. Il peut y avoir également arythmie entre les diverses images visuelles, entre l'image visuelle et l'image sonore, entre les diverses images sonores; cette polyvalence de la projection a pour effet d'obliger le spectateur à chercher à sortir de l'ambiguïté determinée par cette arythmie, à «comprendre» (en un mot assez imprécis) le film; comme je l'ai déja dit, c'est cette activité du spectateur qui vivifie l'intérêt; le spectateur peut se dire à la fin du film que les événements se sont passés tout à fait comme il le prévoyait, ou bien comme il le désirait, ou comme il le craignait.

C'est d'aoir remarqué que cette liberté et cette ambiguïté sont des conditions fondamentales pour que le film intéresse et que le spectateur ne reste pas passif mais actif lorsqu'il assiste à sa projection, qui m'a amené à rechercher expérimentalement

si et comment le film peut être utilisé comme procédé de la technique projective.

Il ne m'est pas possible dans ce court article de m'étendre pour montrer en quel sens j'emploie l'expression: «méthodes projectives». Je dirai seulement que je laisse de côté toutes les acceptions plus ou moins liées à des conceptions doctrinales. Par exemple on peut dire que celui qui le premier a utilisé les méthodes projectives et en a fait peut-être le plus large usage est Freud; sans aucun doute, dans les méthodes projectives apparaissent principalement des processus inconscients; mais ils ne sont pas seuls; d'autres interviennent aussi, comme l'imagination, la mémoire, etc. Le caractère fondamental des diverses méthodes projectives est de résoudre la tension du sujet; c'est pourquoi Cattel [2]) préfère l'expression «dynamism Tests». Je préfère me dégager de tous les points de vue doctrinaux et utiliser, ainsi que le fait Bell [3]), "The most common meaning of "projection", stemming from the latin roots, to the use of the word. In this sense, it means "to cast forward", which is the action involved in the techniques. The subject manifests hid personality in them by "thrusting" it out where it may be inspected. In the "throwing" the personality is not grossly modified; it is only externalized in behaviour that is typical of the individual".

Donc la technique projective agit comme «agent catalyseur» provoquant la réaction, pour utiliser l'expression originale de Frank [4]).

Voici comment je procède. Grâce à la méthode des cartons animés, je construis des situations ambiguës, semblables à celles qu'utilise la méthode T.A.T. et qui se sont révélées fécondes. Voici quelques exemples: 1) un homme ouvre la porte d'une chambre; il passe la tête dans la chambre; celle-ci lui apparaît comme vide; 2) une femme dont on voit seulement une partie, la tête, mais qui suffit à faire comprendre qu'elle appartient à une certaine catégorie de personnes, s'approche d'un homme

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[2]) *Projective and the Design of projective Tests of Personality*, "Char. a. Person", 12, (1944) 175 et ss.

[3]) *Projectives Techniques. A dynamic approach to the Study of the Personality, London*, 1948. (Voir l'introduction).

[4]) *Projectives Methods for the Study of Personality*, "Journ. of Psych." 8 (1939), p. 389 et ss.

courbé sur son travail; 3) une femme se jette sur un divan et pleure convulsivement, etc.

Ces projections ne durent que quelques minutes, et laissent le sujet dans un état d'ambiguïté. (Pourquoi la chambre est-elle vide? La femme invite-t-elle l'homme ou est-elle indifférente? Pourquoi l'autre femme pleure-t-elle? etc.).

Jusqu'à maintenant j'ai pu faire quelques essais, mais non étendre systématiquement mes recherches en examinant de nombreux sujets; aussi dois-je me limiter à une sommaire communication.

Mon but, ultérieurement, est d'examiner des délinquants, des malades mentaux et des enfants arriérés; mais si fragmentaires que soient les résultats obtenus jusqu'ici, ils sont suffisants pour démontrer l'efficacité de la méthode et la supériorité de cette technique sur la technique commune du T.A.T. dans laquelle on présente au sujet des planches où sont représentées des situations en blanc et noir. En effet, dans le film, l'ambiguïté du sens est plus évidente, et par suite la liberté dans laquelle est laissé le sujet est plus grande et le sujet peut interpréter ce qu'il a vu sur le film et nous en donner une «histoire».

Il résulte des mes essais déjà effectués que les sujets projettent bien dans les personnages présentés dans des films courts leurs états d'âme et surtout leur monde inconscient.

Si je me limite à cette communication sommaire, me réservant de revenir sur le sujet lorsque j'aurai pu achever l'examen de nombreux sujets, il me semble pourtant que je puis conclure que la projection de films peut être utilisée comme perfectionnement efficace et utile de la technique commune des méthodes projectives et principalement du T.A.T., dont on connaît l'utilité comme test pour l'exploration de la vie inconsciente.

Le sujet vit donc, pendant la rapide projection des films et avant de décrire ce qu'il a vu, comme deux vies; d'abord la sienne propre, mais aussi en même temps celle du personnage représenté dans lequel le sujet projette (déplace) ses propres sentiments, ses tendances, ses états d'âme. Le sujet n'est plus spectateur de l'action qui se déroule devant lui, mais participe activement à cette action. Dans cette manière de vivre une action, très souvent la tension individuelle (ou un état d'angoisse) se résout. Aussi je pense que le film, développé de façon

opportune en méthode projective peut servir aussi de méthode curative pour résoudre certains états d'âme, vaincre des résistances intérieures, rendre conscient ce qui ne l'est pas, en un mot libérer le sujet de la situation dans laquelle il est comme lié et comprimé [5]).

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[5]) Le Prof. Fulchignoni a utilisé le film comme méthode projective. Son travail vient d'être publié dans l' "Archivio di psicologia, neurologia, psichiatria" dirigé par moi (A. XI, f. II), *"Il film e i metodi proiettivi"*.

SOME PRELIMINARY REMARKS TO A METHODOLOGY OF PSYCHOLOGICAL INTERPRETATION: ON "FALSIFICATION"

BY

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1. *Psychology Studies Human "Behaviour"*

When William Mac Dougall proposed to replace the older verbal definitions of the field of phenomena studied by psychology by his "human behaviour", as the object of psychological science, there was much approbation. If compared with such formulations as "the science of the phenomena of consciousness" or "the mechanisms of the mind (German: "die Seele")" it showed the obvious advantage of pointing to (intersubjectively) observable facts, that could really be studied systematically. In many respects MacDougall's definition has proved its fruitfulness.

Yet it appeared to be rather difficult for psychologists to live up to this position. Certainly the most straightforward endeavour to do so came from Behaviorism. It had the great merit of demonstrating most convincingly to what excesses of agnosticism we are obliged to proceed when trying to exclude any sort of interpretation from the observation of behaviour. Verbal description (e.g. of observed movements) being almost impracticable to this purpose, Behaviorism promoted the use and the development of such experimental techniques as photographing, filming, (wire-)recording. But like other "objective psychologies" (e.g. the Russian reflexiology of Pavlov, Bechterev and followers) it proved unable to contribute much to our understanding of the "higher" forms of human behaviour. The main cause of this failure might be its too rigid, negative attitude towards some fundamental methodological difficulties. Especially the problem of how to control implicit interpretation in common human observation of human behaviour, was avoided, not solved by behaviorism. The same obtains with the still bigger question,

how to place the results of introspection in the "science of human behaviour".

At present only few psychologists still aspire to ignore on principle all evidence from introspection. If psychology wants to penetrate the higher forms of human behaviour it just has to listen to the contributions to the problem furnished by the subject himself. Psychologists cannot afford to avoid all implicit interpretation either, but they must know precisely where observation ends and interpretation starts.

So the problem of what is to be considered the object of psychology, can be restated in the following way: We want to keep to MacDougall's definition, at least to its stress on observable behaviour, but we reject the consequences drawn by extreme behaviorism. There is, apparently, only one sensible way out of these difficulties, namely a widening of the concept "human behaviour" [1]). This can be easily achieved by *including productive and expressive acts of behaviour, verbal or non-verbal, and the results of such acts.*

If taken in this sense, human behaviour can remain the object of psychological study, and of any psychological interpreting activity, being an important part of this study, as well. So we may interpret some particular action or reaction, or only some part or aspect or result of it; or we may interpret some observed habit or attitude of a person, being a repetitive act (or part) of behaviour; we may interpret his Rorschach-protocol, being a result of behaviour (in experimental conditions), or the essay the subject has written, or his handwriting, or a picture he may have painted, and so forth. Sometimes the procedure is a little more complex. Test behaviour in paper-and-pencil ability tests for instance is seldom directly interpreted. Beforehand items are corrected, points are counted, results are calculated in a way as to facilitate statistically reliable comparison. But this is only rearranging the given materials. Provided that no spurious factors are introduced it is still results of behaviour that are interpreted.

But in case of a dream or phantasy, where are the observable facts — acts of behaviour or results of these — to be interpreted?

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[1]) Such a widened sense, besides, might be better in accordance with MacDougall's original intentions.

Here obviously we cannot do without the subject's verbal, or possibly pictorial reproduction of his inner happenings. Dreaming is no perceivable behaviour, but telling about one's dream — maybe distorting its contents — is. So this communication, in one form or another, can be considered the "act of human behaviour" to be interpreted. Inner life, that finds no expression whatever, is not interpretable. Only by some act of behaviour it can become so [2]).

2. *What is Psychological "Interpretation"?*

What do we do and what do we aim at, while interpreting? We try to understand a subject's actual behaviour as a result of the interaction of the situation and his personality. His acts of behaviour in this particular situation should be in accordance with our general "picture of his personality"; they are interpreted as a result of it. This looks like induction and deduction; as a matter of fact, it is. Tomkins, in his work on the T.A.T. (17), observes quite correctly that interpretation as such "is an activity common to all science. Unless it is our assumption that the interpretation of psychological data necessitates principles of inference peculiar to psychology, we must adopt the methodology common to all inductive and deductive science" (p. 43). The psychologist who, starting from symptoms, facts of life, test-scores, observational data, experimental productions etc. of a subject, tries to design a picture of his personality, proceeds typically from special facts to general regularities and to (psychical) causes: his reasoning is inductive. The one outstanding difference with most other types of inductive reasoning in science is that the field of phenomena, to which in our case all generalisations, hypotheses or even theories refer, is restricted to

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[2]) Some difficulties might arise in the case of a psychoanalyst analysing mentally his own dreams. This is interpreting of course, but is does not fit in our definition apparently. But here we should keep in mind that we are trying to define the object of psychological interpretation as a *scientific activity*. As soon as our psychoanalyst pretends that his interpretation has scientific value he can only prove it by "behaving", in our acceptation. As soon as he wants to join in the game of science with his interpretation, he must communicate to others in some way both the dream and his analysis, as Freud, for instance, has done. So here, again, scientific psychological interpretation, whether by self of by others, can start from behaviour.

the personal behaviour, former and future, of one individual. The regularities are expected to be found in the behaviour of this particular subject; the causes are supposed to lie "in" his personality. "Studying mr. X's unique personality" is identical with: "studying the unique field of those events and objects in which mr. X is personally involved". The personality-concept "defines" a special field of (intersubjectively) observable phenomena, and is, inversely, defined by it.

This "field of phenomena" is of course the subject's "behaviour", from birth to death. Acts and results of behaviour are the *primary* material the psychologist has to deal with while trying to study to some purpose a subject's personality. And again, the material to which his interpretations are *finally* referring is the subject's behaviour, former and (possibly) future.

So what, then, are we doing when designing a picture, in words, of a subject's personality? We are *venturing an interconnected set of hypotheses on his behaviour, former and future.*

Psychological interpretation, proceeding from the facts of actual behaviour to personality, can thus be considered a special case of induction. So Tomkins **(17)** is right to revive the principles of induction set up by John Stuart Mill: they can very well serve as a working basis for case interpretation. But they can never prove scientifically the interpretation actually given. There does not exist any way of proceeding with logical certainty from special cases to general hypothesis: the latter has to be "found", to be "ventured", and then, time and time again, to be *tested* whenever possible. The final proof of any hypothesis in empirical science (including psychology) is indeed in the testing of its consequences. Induction being a possibly guidable but in the last resort logically uncontrollable process, the possibilities of subsequent deduction and testing first and foremost deserve our attention.

3. *The Criterion of Falsifiability*

In empirical science "the sense of any statement is the method of its verification" (cf. **12** p. 69). Such was the opinion of Moritz Schlick, one of the founders of the former "Wiener Kreis", precursor of the "Unity of Science Movement". This is certainly exaggerated, especially as far as it should imply the

complete senselessness of all statements not allowing any immediate deductions of testable consequences. Another weakness in Schlick's position has been revealed by K. R. Popper (13): no general principle, no law of nature, no causal interpretation, no general statement can ever be "verified". It can be tested endlessly, always with positive results — which do however, not logically prove its truth. One negative observation, if reliable (repeatable), still will suffice to refute it, or at least to necessitate changes in its formulation. So there is a logical asymmetry between "verification" and "falsification". The demand for verifiability of a statement cannot be used universally, the demand for falsifiability can.

On the strength of these considerations a revision of Schlick's formula is proposed here, namely, cutting down the exaggeration, replacing verification by "falsification". Specializing, then, for psychological interpretation we would suggest: *The scientific value of a given psychological interpretation depends largely on its liability to empirical refutation.*

Phrased in this way the formula can be used as a very important methodological device by the psychologist. It should be for the interpreting psychologist a point of honor indeed, as it is for the physicist, for instance, to express his views in terms indicating possible deductions that can be empirically tested. He should even like to risk the refutation of his hypotheses on the subject's behaviour (personality), nay, more, he should himself seek and set out the ways to do so.

There exists, incidentally, not only a logical but also a psychological asymmetry between verification and falsification, that provides an additional argument for stress on refutability. Neither theorist nor practitioner need to be much urged to look for facts "verifying" (better: supporting) their views: they will quite naturally want to do so. But facts incompatible with their theories or interpretations will more probably be overlooked in some way or another. Charles Darwin is reported to have developed the habit of noting down in a special notebook all observations and facts apparently contrary to his favourite theories, for he knew he would otherwise surely forget them. That shows an early insight in the mechanism of "Fehlleistungen" and, moreover, a good example of the scientific attitude: first and foremost the possibilities of refutation should count.

4. *Factors Reducing Falsifiability of Statements on Personality*

a. A subject's behaviour begins with birth and ends with death. This sets rigorous limits to the possibilities of testing the hypotheses implied in any personality picture. The field of phenomena, to which these hypotheses finally refer, is limited. Of all courses of life, possible from the standpoint of personality, only one line, through time, is realised. Now most psychological predictions, following from some personality-hypothesis, cannot be but conditional: they depend as a rule on the interaction of the subject with a social situation. So if such a social situation never comes to existence, if the conditions never arrive, if the alleged potentialities get no chance whatsoever to be developed, the related predictions cannot be tested. A psychologist may predict that subject A will not fit in a certain job; that B will ruin the life of his future wife, when married; that C, if properly guided and trained, will become an apt scientist; or maybe that D, on his arrival in the U.S.A., will try by all means to embrace the statue of Liberty. But now suppose that A does not get the job, that B does not marry, that C never gets any proper training and that D never in his life visits America — then nothing can be proved nor falsified.

b. The personality of a subject is not a static entity: it grows, it develops, it changes. In the here adopted terminology this amounts to the statement, that many hypotheses on behaviour are meant to be valid only during a certain period or phase in the subject's life. The duration of this period may vary from some days (e.g. the acute phase of some emotional problem) to a lifetime. Anyhow, as soon as the period is over, the related predictions are no longer testable. So here we find a further restriction of the possibilities of falsification.

c. Then problems of a very fundamental nature arise in the testing procedure itself. Even hypotheses leading to quite concrete predictions on behaviour can hardly ever be falsified easily. When the subject does not do what, according to these predictions, he is expected to do, it is still possible, usually, to recur to "disturbing factors". Some of these possibly disturbing factors are indeed well known and dangerous enough, for instance the influence of the observer on the behaviour of the observed subject (including suggestion), or the effect of other

unconsidered aspects of the situation. On the other hand some complex personality factor may interfere; motives other than those considered, possibly unconscious ones, may be responsible for the resulting behaviour.

So the situation is rather complex. A hypothesis on personality can, theoretically, be falsified by falsifying some of the predictions derived from it. But these predictions are conditional, and it proves very difficult to make sure that all conditions — explicit and implicit — are fulfilled. Very often recurrence to "disturbing factors" is possible. In any case a careful *interpretation* of the non-arrival of predicted behaviour is needed, before this non-arrival can possibly be considered to falsify the underlying hypothesis. So the seemingly simple activity of falsifying a hypothesis primarily amounts to painstaking reasoning, in order to demonstrate convincingly the impossibility of evasive interpretations. This necessity is not peculiar to psychology of course, but the multeity of factors, whether "disturbing" or not, is. In psychology even the most simplified, experimental conditions do not allow that isolation of psychical structures, as would be needed to get really univalent results; let alone, that the much less controllable conditions of common observation would do so.

d. Problems of a special nature arise as soon as the "Unconscious" is involved. A statement about a subject's conscious motives, wishes or inner conflicts can often be refuted by careful interrogation, but in the case of unconscious motives etc. this direct way is blocked. Yet Freud himself tried eagerly to work on an empirical basis. He taught his followers not to impose an interpretation, but to guide the subject to finding it himself. The approval of the subject, anyhow, was of great value to him — at least if the subject did approve! As a matter of fact the approval of the subject is considered by Freudians, sometimes rather regardless of the possibilities of suggestion, to support, if not to justify the interpretation given; his disapproval, however, is hardly ever considered to weaken it, let alone to refute it. Ignorance about his own inner life, together with unconscious resistance may prevent the subject from perceiving the truth. Even if we do not agree with the extensive use made of this argument by Freudians for the sake of self-defence, and indeed we don't, it must be acknowledged that it can be quite correct.

So we are again devoid of any reliable method of falsification.

Moreover falsification with the help of the subject is altogether impossible as regards the analysis of products of the human mind — in the case of dreams, fantasies, or myths, ancient literature, etc. — whose authors can no more be asked questions.

e. Apart from the problems of exploring the "Unconscious", the lack of falsifiability in the analytical psychologies has other, still more fundamental aspects. As is well known, C. G. Jung, deliberately abandoning Freud's, in his view too narrow, "attitude of a natural scientist", does not consider the agreement of the subject of much importance. Instead he relies rather exclusively on piling up manifold analogy as a method of justification. By this method, when handled skilfully, a given interpretation can indeed be often supported convincingly: but it can never be refuted. Jung does not seem to have bothered much about this, now complete, loss of falsifiability. Probably he felt this to be only a consequence of the fact that *practically every pattern of behaviour* or product of human psychical expression, such as studied by psychoanalysts, *allows more than one sensible interpretation* [3]). This "multiple interpretability" is not only a result of the deficiencies of our empirical materials and of the weakness of our interpreting and testing methods. It also reflects certainly a real *multiplicity of sense*, that can be explained only as an effect of a multiplicity of motivational sources, conscious and unconscious, in the subject's mind.

As a matter of fact it happens very often, that not only separate acts but even extensive patterns or products of behaviour of a subject can be interpreted, convincingly and in detail, from two or more widely different points of view. If these interpretations are both consistent with all available facts, and if they are not logically inconsistent with each other, they may quite well be both justified. Also, such a state of things is very much in accordance with general introspective data on the possibilities of multiple motivation. In fact we have every reason to believe, that psychological causality, so to speak, preferentially weaves several patterns over one another into actual behaviour, especially so into "expressive" behaviour. This reflects the fact,

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[3]) This fact was already noticed by Freud. But it was Jung, who stressed its general significance.

that the analysis of personality — that is of personal behaviour — is essentially and unavoidably multi-factor analysis.

These considerations complicate the whole problem of falsification. Such coexistent, not necessarily inconsistent and non falsifiable interpretations, besides, may quite well differ largely in explaining capacity. One view (interpretation, hypothesis) may be "more justified", "more clarifying", "more important", "more comprehensive", "more exhaustive", than another. One interpretation, taken apart, may be more "one-sided" than another; although, strictly speaking, they must both be so. The several interpretations complete each other; each of them represents "a part of the truth", an "aspect" of reality. These wellknown expressions introduce implicitly a sort of *relativation of truth*. Obviously we can not do in psychology with the simple distinction between "true" and "false" interpretations. Apparently there are, at least, extensive regions in psychology, where the absolute criteria of falsifiability and falsification, however important they may be, can hardly be applied. In these regions the elaboration of other methodological criteria, allowing *relative judgments* on the value of interpretations or hypotheses on behaviour, is of the utmost importance. In this paper, however, we can not yet enter upon a discussion of such criteria. — Let us summarize in a few words the contents under this heading. We have discussed several factors reducing the falsifiability of hypotheses and interpretations regarding human personal behaviour:

a. *the limitations* of the field of phenomena, to which these hypotheses and interpretations finally refer: *human life*, from birth to death, realising only one line of developmental possibilities and excluding repetitive experimentation;

b. the *development of personality* itself — or, in other words: the fact that many hypotheses and interpretations can only be meant to be valid during certain phases of life;

c. the impossibility of eliminating from observation and even from experiment the influence of possible "disturbing factors" or, in other words: *the impossibility of isolating* both environmental influences and motivational factors (cf. *e*), that is, of *isolating psychical causal structures;*

d. the special difficulties, as to falsification, brought about by the introduction of *unconscious* motives, wishes, conflicts, etc.;

e. (underlying in part *c* and *d*) the essential, unavoidable *multi-factor determination of human behaviour,* especially so where expressive (possibly "projective") products are concerned, such as analyzed by depth-psychology; to the effect, indeed, of establishing "multiple sense", "riches of aspects", etc. of these products. —

So we see that the testability of hypotheses on individual behaviour can not be but restricted. This is a serious setback to all endeavours to handle the problem of interpretation scientifically. It should not lead us, however, to neglecting the primary importance of the principle of falsifiability.

5. *Terminological Problems with Regard to Falsifiability*

According to the here adopted point of view a verbal picture of somebody's personality is essentially an interconnected set of hypotheses on his personal behaviour, former and (possibly) future. But does the verbal expression of these hypotheses, as found for instance in some written personality report, actually speak of acts of behaviour, former and future, or of results of such acts?

Only seldom concrete (predictive) statements are found. Generally a more or less "intrinsic" terminology is used, designating defects, abilities, intelligence, talents, temperamental and character-traits, tendencies, sentiments, attitudes, inner conflicts, complexes, needs, motives conscious and unconscious, and so forth; all supposed to lie "in" the subject's personality.

Sometimes even several acting centers "in" the subject's mind are supposed to strive and act more or less independently, to entertain mental "tensions" and "conflicts" and to be "structurally" related to each other. According to these notions the human mind would have a rather definite "structure", that might most adequately be discussed in terms of spatial relations [4]). Anyhow, the unbiased layman would hardly learn from the reading of such a report that psychology is the science of human behaviour.

Of course most of these intrinsic ways of expression have their merits. They can certainly be fruitful and elucidating sometimes. But they have one very dangerous quality: they are apt to suggest that they represent a reality of their own without any

[4]) Some authors bring, indeed, quite a geography of the psyche (cf. 4 p. IV and 8, with many diagrams).

connections with the perceivable behaviour of the subject. They are apt to make the psychologist believe too concretely in the "existence" of psychical instances, forces, mechanisms, relations and structures, and to make him forget their hypothetical, often highly metaphorical character. Whoever speaks this intrinsic language habitually, risks to lose some day his skill in "translating" his verbal statements critically into the behaviour terminology. He risks to lose his ability in walking the logically legitimate way to observable facts and testable relations, back and forth.

Obviously any unnecessary terminological "intrinsicism" endangers the principle of falsifiability. This can best be demonstrated by eleborating somewhat on the possibilities of *hiding in terminology*. It is rather common knowledge (cf. popular astrology) that it is possible to compose characterological passe-partout reports, that 9 out of 10 people would gladly and unsuspectingly accept as a picture of their own personality. A preliminary analysis of these hiding techniques (cf. 7) has shown, that there are several means to evade the risk of falsification:

a. by avoiding extreme statements, inserting "rather", "a bit" etc.;

b. by elaborating on some favourable social traits, that are practically universal among the group or class, to which the subject belongs (e.g. honesty, sociability, etc.);

c. by giving "deep" (causal) interpretations instead of descriptions and expectancies regarding concrete behaviour;

d. by employing the interpretational scheme of conflicting psychical forces — resulting in nothing testable (e.g. passions mastered, emotions governed, nervousness controlled, etc.);

e. by using generalizing terms, that may apply to several diverging fields of human behaviour;

f. by preferentially using, for purposes *c*, *d*, and *e* especially, difficult technical terms.

Of course these points are not meant to serve as a primer for charlatans. They only indicate, how in applied psychology — as in philosophy, and in diplomacy — the obscurities of language can be misused for hiding and for taking cover, unconspicuously. Every scientifically minded psychologist, in order to avoid unintentional slips, should be well aware of these possibilities.

To our purpose the points *c*, *d*, *e* and *f* are most interesting, for they have to do with "intrinsicism". Points *c* and *d* do not need much elucidation. The "deeper" the interpretation, the longer is the way to testable facts and the greater is the chance that added vaguenesses on this way will at last efface every empirical content. Point *d* will remind the reader of Klages (cf. 3 p. 273); his interpretational scheme of conflicting forces, functionally clarifying though it may be, can indeed very easily be misused (often unintentionally) for hiding.

Some more attention must be paid to points *e* and *f*, which pretend somewhat surprisingly that the use of general and technical characterizing terms (e.g. "introvert", "egocentric", "integrated", "emotional" and many others) as a rule does not benefit falsifiability. There are indeed some indications (cf. 7), that characterizations of persons by such terms are less intersubjectively reliable [5]), than more or less corresponding terms in common speech. In order to understand this, it should be remembered, that these technical terms mostly refer to fundamental distinctions from a functional and/or introspective point of view. This does not at all imply that they should be just as suitable for exclusive personality descriptions. They are not, as a matter of fact, because we are, all of us, *in some respect* egocentric or emotional or introvert. Trait-names in common language generally designate pregnant, clearly distinguishable forms of behaviour, whereas psychological terms are apt to be more intrinsically defined. That does, of course, not invalidate them completely; it only amounts to a weakness in power of distinction. If used without specification, they can not easily be falsified.

In any event we should not indulge in the fallacious belief, that the introduction of technical terms, per se, brings about clarification and precision. Some authors, bombarding their readers with neologisms and other technical (Latin) terms, used in a special but never clearly defined sense, seem to be victims to this belief. Like system-producing philosophers they often employ quite a vocabulary of their own, that makes their

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[5]) Only if some intersubjective reliability is assured, it can possibly come to falsification. For the rest the methods and criteria for falsification of characterological statements are still to be established (cf. 9).

books unnecessarily difficult reading (**16**), if not completely unreadable (**19**). This is a very deplorable practice, indeed. In the present rather chaotic state of characterological terminology some parsimony in coining new terms is more than ever needed. Productive psychologists should stop to meditate a moment on the scientific responsibilities involved, before having their personal language printed [6]).

Complicative terminology is probably only rarely used as a hiding technique on purpose, but in its effects on the falsifiability of statements it is such.

6. *"Objectivism": Ignoring the Unique Personality*

The above discussed difficulties in testing statements on personality have led many psychologists to restricting themselves to an "objectivist" approach of personality problems. There are of course several schools of "objectivism", built upon different methodological principles (behavioristic and other), but having one thing in common: the abstention, on purpose, from venturing any personality hypothesis of an individual character.

Instead objectivists rely on more or less general regularities and laws of human behaviour, preferentially on such having a quantitative, statistical nature. These empirical regularities enable them to predict on a probability basis, they allow "the prediction of normative behaviour, of what the "average", "normal". or "typical" individual or even the "typical ten-year-old" [7]) may do under a given set of circumstances" (**15**, p. 7). They do not venture any really individual prediction, they only discuss averages and statistical expectancies.

Snygg and Combs ("Individual Behavior", **15**), in a clear discussion of the weaknesses of the objectivistic approach, refer to it as the "traditional frame of reference of psychology" — that is: traditional in American psychology. They remark that

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[6]) Whoever has worked in any rather unexplored field knows of course that the introduction of new terms and concepts — both descriptive and explanatory — often is absolutely necessary for progress. But this should be done very carefully, in such a way, that its applicability to special cases can at least at some critical points be empirically tested. (cf. 6)

[7]) The reader may get an impression of the tremendous amount of data gathered by objectivistic research — and of the essential limitations of the method, too — from many American publications, e.g. Bentley's "Superior Children" (mostly on the "typical" superior child).

the simple type of causal relation underlying most statistical predictions, i.e. "because something has happened so many times in the past it will happen so many times in the future" can only be applied with certainty if there is no reason to suspect any slight differences in the circumstances or in the group whose behaviour is predicted. And there is certainty but seldom. Moreover, "in the great majority of situations this prediction of normative behaviour is not enough" (p. 7). Particularly is it not enough when we deal with individuals; as in most cases we do. A psychologist can hardly base "his prediction of what Oscar Jones will do in a given situation on what the "normal" or "typical" person — or maybe the typical adolescent, the typical 19-year-old or the typical college freshman — has been found to do in that situation." Even the most extensive body of factual knowledge of such a general nature can never provide sufficiently conclusive arguments for the prediction of individual behaviour. So if a psychologist does predict on this basis, he does so "with the knowledge that Oscar almost certainly differs from the norm in some way or other and consequently will not do exactly as predicted". (p. 7—8).

In other words, he does *not* venture any hypothesis on individual behaviour. For we should keep in mind that statements like: "Mr. Jones has — with his age, his IQ or any other factors considered — a chance of 2 out of 3 to succed in college" is not a statement about the individual Jones. It is only an abbreviation of the general statistical expectancy, that, in a group of numerous individuals of the same age, IQ, etc., about two out of every three will succeed in college as against one who will not. As soon as we consider Mr. Jones an individual — we have, for instance, come to know him personally — the above cited form of the statistical statement can no longer be maintained; maybe Mr. Jones personally has, in spite of his age, his IQ and other factors, "no chance at all" to succeed (cf. 1 and 2).

Objectivists thus abstain from any personality hypothesis of a really individual character. They want to refrain by all means from metaphysics — and therewith from non-quantitative data and from any use of intuition. They do not trust only verbal, not numerically expressed, psychological laws, let alone subjective personal experience or impressions. As a consequence objectivists naturally tend to employing objective, measuring

methods; they are primarily interested in capacities, intelligence and other "measurable" qualities, regarding which individual interpretation can be reduced to a minimum. They do not try to find the core of human personality nor do they speak such an intrinsic language as "subjectivists" do. Objectivists talk, often not even behaviour, but mainly statistics and numbers, qualifying alleged capacities and their correlation.

The objectivistic attitude, maybe a little exaggerated here for the cause of exposition, is of course well known, not only in America, but in Europe as well. But from the methodological point of view here adopted, which should be our position regarding "objectivism"?

We cannot blame, of course, the objectivist for his modesty (unless he be not as modest as he seems to be). There is only one thing the objectivist might be blamed for, namely his attitude towards the individual personality. While endeavoring to deduce all personality-traits from more or less general relations and while refusing to venture any unique hypothesis on individual behaviour, in a way he ignores the individuality of his subjects itself. This, however, is a point we want to stick to, in any case. We want to consider — perhaps inspired by some democratic feeling — every human personality as unique and therefore necessitating finally individual understanding, approachable only by an *individual* "set of interconnected hypotheses on behaviour, former and future".

7. *"Subjectivism": Neglecting Testability*

The above repeated criticisms on objectivism are far from new. Some twenty-five years ago German and Swiss psychologists (e.g. 9) had already found it impossible to approach via behaviorism and statistics the essence of personality, that is: the final motives and dispositions governing actual behaviour. Unfortunately many of them fell a victim to some other opposite extremism, to some form of "subjectivism".

Subjectivist schools of psychology tend to reject on principle all measurement in psychology. They prefer methods allowing some direct intuitive attack on the "deeper layers" of personality, i.e. on the individually characteristic pattern of tendencies, motives, sentiments and dispositions supposedly underlying the subject's actual behaviour. Working in a scientifically

still rather unexplored field many subjectivists built deliberately on metaphysical grounds.

As a matter of fact subjectivism in European test-psychology is to be considered only one manifestation of a more general anti-rationalistic trend. Mainly in German and Germany-influenced cultural spheres the differences between natural and cultural sciences ("Naturwissenschaften und Geisteswissenschaften") have often been highly emphasized. It has been claimed that the "Geisteswissenschaften" should develop their own methods of observation, analysis and reasoning independently from the older examples set by the natural sciences — which is not an absurd but surely a very dangerous starting-point. Thus in test-psychology high level intuition ("Verstehen", "Wesensschau", "Einfühlung") has been alleged to be the pre-eminent method of psychology as a cultural science ("geisteswissenschaftliche Psychologie").

The main argument in favor of well trained intuition, endlessly repeated in subjectivist publications, is of a negative nature: since objectivism is unfit to uncover the radicales of individual personality a new approach is needed. Often not only objectivistic methods were banned, but also the natural-scientific attitude or even science in general (Klages) — in order to clear the way for metaphysics.

The fact that these attacks on rationalism often degenerated into deplorable attacks on reason, should not blind us for their initial merits. Notably in psychology many schools of thought seem to have the essential historical mission of overcoming some unspoken rationalistic or materialistic prejudice. Thus Gestaltpsychology, for instance, overcame the prejudice, that psychical phenomena, in perception, could be isolated, added and divided as can be done, sometimes, with the corresponding physical stimuli. The most essential contributions of "Ganzheitspsychologie" and of "Phänomenologie" could easily be formulated in an analogous way. Perhaps it is necessary that the road to a really psychological empiricism, free from inadequately borrowed methods and conceptions be paved with "schools". In any event there still exist many rationalistic prejudices to be overcome; a recent example is Roethlisberger's famous experimental uncovering of the "human factor" in industry (14).

To be sure the prejudice attacked by subjectivism in test-

psychology is rather obvious. Subjectivists deny expressly that the maintenance, out of sheer objectivistic modesty, of a professional ("natural-scientific") attitude of humanly disinterestedness can ever lead to understanding human personality. So far they are right, of course. And their fight for a more personal participation in the individual case studied — in order to "measure" the subject's personality by the *personality* of the examiner, to borrow a phrase from Carrard — has been fruitful. Subjectivists have developed "structural understanding" ("Einfühlung", etc.) as a practical method, they have contributed to designing projective techniques, to elaborating interpretative methods, and they have demonstrated the possibility of systematically building up one's observational and intuitive powers. All this is certainly important. Its importance would not diminish, even if the argumentation underlying the views of the several schools of subjectivism woud prove altogether highly metaphysical. As a matter of fact this might well be the case.

We cannot enter here upon a detailed discussion with the different schools of subjectivism. Only some general, rather serious sentences must be pronounced:

1. Subjectivism's complex justifying argumentation in favor of intuitive understanding is, from the standing-point of theory of knowledge, rather *unnecessary*. Fortunately there exists no law of logic nor any principle of empirical science forbidding the use of intuition. There exist no logical rules governing the way of *finding* hypotheses — and it is as hypotheses on individual behaviour that the content of a report on personality should be considered. Only after hypotheses have been found and formulated can logic and theory of science start criticizing and demanding. The main argument then may evolve from the outcome of the testing of results. For instance, when a charlatan, diagnosing with some quacking technique, can show really dependable results much superior to those of academic psychology, it follows that his technique and intuition combined are in all likelyhood no pure quacking. In such a case it might pay official science to find out how he does what he does, in order to possibly systematize his method, and even to adopt it. At any rate, there is no reason whatever to ban a priori the use of intuition; it needs no philosophical defense.

2. Subjectivism's emphasis on intuition ("Verstehen", etc.) is

also rather *misleading*. For the attacks on objectivism and the complex metaphysical argumentations in favor of intuition as a method, form, so to say, a smoke screen, hiding from view the one great problem of validation! Here is the real weakness: subjectivists, as such, have never greatly applied themselves to objectively dependable testing of their results. They seem to consider their subjectivism a valid excuse for this neglect, but this is certainly a wrong opinion. For no stress on any method of *finding* hypotheses (i.c. interpretative or therapeutic possibilities) can ever minimize the scientific necessity of *testing* them. On the contrary: *the more subjective the technique of interpretation and the higher its pretensions, the more forcing is the exigency that results be critically tested.* Unfortunately enough, this conclusion seems to be hardly ever acted on [8]).

3. Subjectivism's lack of interest in validation problems — well trained intuition ("Wesensschau") being considered the one dependable guide — is often reflected in *subjectivist language*. Extreme representatives like Klages do not care if they use a highly complicated, hazy, metaphysical, intrinsic, personal language, preventing "falsification" to a high degree. From our point of view this must be considered a most fundamental weakness: by neglecting not only practical validation but also the terminological exigencies of falsifiability (that is „validatability!") subjectivists do leave the domain of empirical science.

8. *Several Ways from Observed to Predicted Behaviour*

"Behaviour" has been defined here in an unusually broad sense, not excluding but including consideration of (verbal) behaviour in situations requiring any form of introspection. We are now able to elaborate on the statement that *the materials of psychological science* — primary and final materials — *are exclusively acts and results of "behaviour"*.

It is rather obvious that every interpretation and every prediction on individual behaviour must be based primarily on the subject's "acts and results of behaviour", as these have been observed or otherwise registered. The same obtains, indeed, with the prediction of normative behaviour. Psychology as a science

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[8]) Possibly for psychological reasons? The combination, in one personality, of interpretative intuition and push on one hand and of penetrating self-criticism on the other must indeed be very rare!

can only operate on an empirical basis, of course; so the *primary* materials are „behaviour". It is, however, not so generally accepted, that the *final* materials also are, or at least should be "behaviour". Some schools of psychology — not only markedly subjectivist schools — maintain, that the analysis of the *structure of individual personality* is, per se, the final goal of the psychologist. In their view the possibility of concrete prediction is only a consequence of secondary importance, to be drawn from the subject's personality structure as analysed and reconstructed. When these psychologists have worked out the subject's intrapsychical relations, his sentiments, his complexes and inner conflicts; when they have pictured him in terms of capacities, abilities, traits, qualities, tendencies, etc. they consider their task fulfilled.

From the viewpoint of our methodology this is a dangerous position. It leads in psychodiagnostic practice quite easily to an overestimation of the structural concepts by which — in some accepted "frame of reference" — personality has been described. As this description of personality is considered to reflect utter reality, its hypothetical character and the necessity of its validation by new facts are easily neglected or even completely forgotten. The structural description of personality, by induction built on the behaviour observed, may then develop into a "closed" systeem, no longer open to falsification or modification by new facts. The examiner, after having constructed his picture of this structural "reality", may thus lose all interest in the possibilities of translating it into falsifiable predicting statements.

Therefore we have to stress, once more again, the fact that the empirical-scientific value of structural and other intrinsic personality-concepts (including quality- and trait-concepts) depends fundamentally on their "liability to empirical refutation" (cf. 3). These concepts do not reflect the final materials of psychology as an empirical science; they should be considered primarily *vehicles, leading from observed to predicted behaviour*. They are means to an end, albeit often very complicated means to rather simple ends; they are not ends in themselves.

Apparently it is worth our while to trace the passage from observed to predicted behaviour as clearly as possible. The general scheme is rather simple:

1. Observed (or registered) behaviour.
2. Interpretation in the accepted frame of reference.
3. Predicted behaviour.

There are, however, many fundamentally different frames of reference in use; there are several methods to select, to register, to experimentally provoke behaviour; there are different types of inductive reasoning and of inferred causal structures; there are different types of prediction. Only some of the most important ways in use — in passage from observed to predicted behaviour — can be superficially sketched here.

The *objectivist approach* is characterised: 1. by the selection of measurable results of behaviour only; 2. by the express use of statistical inferences from results of mass- or group-research; 3. by predictions on a probability basis, i.e. of a non-individual character. A great advantage of the objectivist approach is its transparency: it is not difficult to follow, and possibly to criticize, the argument from observed to predicted behaviour.

The *"phenomenological" approach,* as advocated by Snygg and Combs, studies all kinds of observable behaviour except (verbal) introspective behaviour which is, rather arbitrarily, completely rejected (15 cf. p. 36). The "phenomenological" frame of reference is used, that is to say: behaviour is explained (and predicted) according to the subject's "phenomenal field", as inferred from his observed non-introspective behaviour. Here, too, the way from observed to predicted behaviour is clearly traced. Especially falsifiable prediction being the ultimate goal of all field constructions, however complicated and interesting they themselves may be, is generally given sufficient attention.

The *approach of depth psychology* (Freud, Adler, Jung e.a.) on the contrary relies very much on introspective behaviour. Most of the materials used by the psychoanalyst for instance to construct his personality picture of the subject — including the analysis of inner conflict — are gathered by means of verbal communication. Special attention is given to special materials (dreams, phantasies, symptoms, emotional aspects of life; as communicated by the subject), and special techniques of evolving actual introspection are applied. Then, according to Freudian or other theory, some hypotheses regarding the subject's stage

of "psychical development", his "inner conflicts", his "motives and tendencies", conscious or unconscious, etc. are formulated. These hypotheses have the double function, here too, of:

1. *explaining* the special facts and aspects of behaviour studied;
2. *predicting* special acts and aspects of the subject's future behaviour, namely: a. his reactions on further analysis and therapy, and b. his behaviour in critical life situations, especially after therapy. Although this aspect is only seldom emphasized, we should keep in mind that every action taken by the analyst during treatment, based as it is on his interpretation of the case, implies a prediction concerning the subjects's behaviour. So the process of treatment implicitly contains quite a series of interconnected circuits: observed-predicted-observed behaviour. But these predictions are seldom clearly formulated, the possibilities of testing (falsifying) them are very restricted, and — worst of all — depth psychologists are hardly interested in these possibilities. Here, indeed, the danger of developing "closed" personality interpretations, neglecting concrete prediction and falsifiability, is very acute.

It would be certainly very rewarding to thoroughly analyse all the different ways the argument follows, in passage from observed to predicted behaviour, in the common *psychodiagnostical practice* [9]). But this would demand a dissertation of its own. Most psychologists will agree without such a thorough analysis, that by far the weakest points are to be found in the second half of these ways, namely in the transition from intrinsic statements on personality to concrete, falsifiable predictions on behaviour and to their validation.

9. *On Validation of Personality Pictures*

We will conclude this preliminary study with some remarks on the problem of validating statements on personality.

1. We can distinguish several types of statements on personality, differing in *psychological content* and in their *logical-syntactical form*. Both factors seem to influence the possibilities and the methods to be used for validation.

Only very seldom unconditional, unique predictions of the

[9]) In Holland psychological examinations on a personality basis — for purposes of vocational guidance, personnel placement, etc. — are widely in use. They take, on an average, about 5 hours of testing and interviewing.

type: "Mr. X will succeed (in a job, in college, for an examination, etc.)" are found. One observation, after some time has elapsed, could verify or falsify such a statement. But it is not a statement on personality. If X happens to die some days before his examination this accident can not be considered to "falsify" any psychological statement. Such a psychological statement would sound: "Mr. X. is able to succeed (in ....)". Now this can still be verified by one new observation: if Mr. X does succeed, he surely was able to succeed. But falsification is more difficult: if he does not succeed, he might have been able enough but for....! So the prediction implied in one statement was already a conditional one. Indicating the conditions of his success can bring some betterment: "Mr. X. is certainly able to succeed if only (conditions are normal, he does his best, he finds some help, etc)". But on the other hand vaguenesses due to the conditional phrasing may spoil the testability completely: "Mr. X is certainly able to succeed, if only he finds the right type of tutor". Without any specification, this formulation opens every possibility to deny — in case of X's failure — that the tutor was of "the right type".

Consider now the negation: "Mr. X. is not able to succeed (in..)" Here the risks, and therewith the possible scientific value of the statement, are greater. Obviously if X does succeed it is refuted. Of course conditions, especially equivocal or unverifiable ones, can weaken the position and avoid risks, e.g.: "Mr. X is not able to succeed, unless (he is helped by others, finds the one type of tutor who understands him, etc.)". Likewise untestable are conditional phrasings like: "Mr. X would (not) have succeeded, had he....". That does not mean, however, that they are devoid of sense. If mr. X would have succeeded, „had he only....", than he might, on a second try, do...., and succeed; if he then fails again, the statement is not directly refuted, but it is nearly so, indirectly.

A special type of implicit prediction is found in statements expressing a person's opinions, strivings, conscious attitudes, tendencies or needs. As far as these opinions, strivings, etc. are considered to be conscious, a validation can be achieved by one careful, direct interrogation. The result of such an interrogation is implicitly predicted; verification respectively falsification can sometimes be achieved. It cannot always be achieved, of course,

in view of the possibilities of partial correctness, relativity, differences in intensity, etc. of the alleged opinions, strivings, etc. Thereby testability is relativated too.

If it can be supposed that mr. X is not completely aware of his personal needs and attitudes, or even if only he is refraining from exposing them, the situation changes completely. Mr. X. might easily agree with a statement that depicts him as " great admirer of Einstein", but be will hardly agree with the sentence, that he is very "self-admiring" or "self-satisfied"; and he will not be a competent judge in the question as to whether or not "he unconsciously admires and imitates his father (although he consciously hates him)". These statements cannot be tested by one observation. They do not predict anything concrete, they claim a certain *pattern* to be present in former and future behaviour. They do formulate a "hypothesis on behaviour" (cf. 2), but they do not indicate any direct way of testing it. So this way still has to be found, when a validation is endeavoured. Generally the procedure will include careful observation and analysis of the subject's (future) behaviour and it will not easily bring about falsifying possibilities. None the less this is a primary thing to be tried, in cooperation with the person who made the statement, preferentially. This person should try to realize which observations he would consider to refute his views, or to force him to partial corrections.

The same obtains with other types of statements on personality. Sentences on a person's special liabilities, distinctive assets, character traits, sentiments and complexes, personal motivation and desires, and on their alleged causal of structural relations [10]), like statements on his attitudes, tendencies and needs, do not provide for immediate clues to their validation. For instance, it would be difficult to find means of testing such specific diagnoses as:

Mr. X "is rather suggestible", "lacks fighting spirit", "is very persistent", "is basically an introvert", "suffers from inferiority feelings", "acts primarily on the basis of his want for personal success", "has made an overcompensation for his character weakness", etc. Hard enough is it to verify or to falsify "ner-

[10]) No effort is made here to systematically classify these different concepts, according to some general theory or preconceived personality structure. They are, however, all of them, current in personality analysis.

vousness", "emotional immaturity", "punctuality", depressivity proneness", or even "masculinity". Many schools of characterology, especially in Europa, have tried to systematize these traits and to integrate them into one structural picture of the human personality (cf. e.g.: **10**). But hardly ever is a systematical analysis and classification from the viewpoint of their testability endeavoured.

Most of these statements can be formulated or reformulated in one syntactical form: "Mr. X is .... (trait-name)", or "Mr. X. has the quality of being....", or: "One characteristic of Mr. X. is .... -ness", or the like. It is, however, just these current formulae, attributing to "the personality" of Mr. X some quality instead of describing his behaviour, which bring about much confusion. In some cases, of course, the statement "Mr. X is ...." may have a concrete, imediately testable meaning. This is the case if concrete abilities are called upon; Mr. X may be, for instance "an apt translater". Or he may be "of superior intelligence". The latter is not one concrete ability, but there exist intelligence-tests and we do habitually rely on them. That is to say: the degree of intelligence of a person can be considered to be *defined* by means of our present testing-technique. This being the case, a statement on Mr. X's degree of intelligence is testable and falsifiable, indeed. In about the same way Rorschach-workers are wont to rely on their findings with regard to introversion-extraversion; their test being considered a reliable testing technique in this respect, the testing method itself has grown a renewed implicit definition of the trait. "Mr. X is an extreme extravert" is a falsifiable statement, therefore — if only the word "extravert" is taken in this acceptation. But these are rare exceptions. When speaking of Mr. X's masculinity or punctuality we hardly refer to some obtained or expected testing result of rather well known practical implications. We do refer to some specific patterns in his normal life behaviour — although unfortunately the logical form suggests an intrinsic quality embedded some way "in" Mr. X's personality structure. This suggestive effect — and not so much the often emphasized fact that quasi-elements are isolated from the unity of personality (cf. **1**, p. 435) [11] — is in our opinion the main danger in the

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[11]) The fact that isolated "traits" have no real sense if taken apart

use of trait- or quality-concepts. They are vague and subjective with regard to their meaning, and they do not themselves indicate anything about the methods of their possible validation. — The above considerations may provide sufficient evidence for the opinion, that *any systematic approach of the problem of validation should start with a classification of the several types of personality statements, with regard to both their logical-syntactical form and their semantical value.*

2. The primary method of validation can only be the search for falsifiable statements in, or consequences from, the personality picture under investigation. In practice, however, personality studies contain only very few clearly falsifiable statements. In view of this fact there exist at least three possible ways to reach higher scientific standards:

a. educating ourselves and other personality-psychologists to more courage and more attention with regard to falsifiability, and to greater skill in translating intrinsic into behaviour statements;

b. studying the possibilities of indirect validation (falsification);

c. elaborating a specialized methodology of psychological interpretation, that employs relativated criteria of verification and falsification (supporting and weakening arguments or facts) and a relativated concept of truth.

One of the objects of this paper has been to draw the attention to point *a.* As to point c, this is too enormous a job to undertake here. So only some words will be devoted to *b;* the possibilities of indirect validation.

Direct validation (verification or falsification) obtains if it is observed, that some implicitly or explicitly predicted event does not happen. This observation ought to be objective and repeatable; it ought to be accessible to any volunteer observer. In the natural sciences these conditions are fulfilled in general; yet the situation is more complicated than it seems to be Often the observation itself is, it is true, accessible enough, but the insight into its significance, especially in its falsifying power, is not. Consider Einstein's Theory of Relativity, where this

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from the whole of personality (from the individual's total behaviour), is not an eternally valid excuse for pronouncing statements that have no real sense if taken apart, anyway.

insight is only accessible to high level physicists. It is to these the man in the street looks; it is these that he believes, in view of their acceptance of it in a body. It is important to note that the argumentation underlying the belief of the man in the street in some consequence of Einstein's theory, is essentially of the type: "It must be true, for all experts say so". We can not maintain the pretention, that he would be able to follow the argument, if properly instructed, for the average man in the street is certainly not intellectually able to do so.

From this point of view the difference between direct and indirect validation — the latter by invoking the judgement of "competent judges" — is not fundamental; if only general agreement can be achieved! Validation by judges is, indeed, the only possibility whenever the statements themselves do not allow the deduction of any directly testable consequence. It does demand, however, a careful and cunning procedure, lest the results be worthless. Unfortunately only litte work has been done in this field: many investigators must have been discouraged by the fundamental difficulties involved.

Yet some techniques have been tried (5, 7). The *matching method* that has already proved a very useful technique in the investigation of the interrelation of different expressive features has also been used for validating purposes (cf. 18). In some cases verbal personality pictures had to be matched blindly either to other verbal personality descriptions serving for criteria, or to the implicit personality pictures in the mind of the judge(s). These methods have some merits, but severe limitations as well. The results of the matching may be satisfactory, surprisingly bad or even embarrassing (7); but neither success nor failure of the method proves or disproves very much with regard to the correctness ("truth") of the separate statements involved. As only verbal pictures or analyses of personality *as a whole* are regarded, the results do not allow a refined analysis.

The method of "overview", initiated by Cronbach (5), has been designed to validate separate statements. A triad of three cases is considered. The personality description made about each case, including more or less concrete predictions, is broken into separate statements. Then three statement sheets are prepared, each of which contains, in mixed order, one third of the state-

ments about each case in the triad. Now these statements are validated in relation to a written criterion description of personality and/or behaviour, by a competent judge. He has to rate each statement as fitting (+) or not fitting (—) the criterion, or as doubtful (o or ?). "Knowing that only about one-third of the statements were made about the man described in the criterion, he must attempt to identify these statements" (p. 368). This process is continued with at least 63 (!) judges, in order to have every statement-criterion pair rated by seven judges at least.

This procedure, it can be seen, is a very cunning one. The results can not be influenced by any bias, they allow statistical elaboration and they indicate separately the accurate and inaccurate statements in the verbal personality picture [12]). The one outstanding disadvantage of the method, mentioned by its author, is, apart from its laboriousness, its limitation to separate statements, isolated from their context.

This disadvantage has been avoided in a study of the present writer (7). In this case the influence of bias of the judges was not avoided: they had to judge according to their own mental personality-picture of the subjects, all of whom they were acquainted with. Only five (grafological) personality analyses were studied, but this was done by a combination of several methods. Both overall blind matching and judgement on separate statements were used, but the latter method was employed in such a way that every statement was *read and judged in the context of the whole analysis.* The judge had to rate each statement as correct (+), incorrect (—) or doubtful (o) with regard to the personality of *each* of the five subjects respectively. This procedure made it possible to compute a "percentage of applicability" of each personality picture to each of the subjects, according to each of the judges. By means of such percentages a quantitative comparison of personality pictures (and subjects) and, on the other hand, of the methods and individual achievements of different analysts was accomplished. So "passe-

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[12]) Further the method may permit a very useful classification of statements with regards to their semantic value. Statements incomprehensible to the judges who will use them, will score many o and ?; or one pair of criterion — statement may score both + and — next to o and ?, if the statement is "unvalidatable".

partout"-pictures could be discarded. Several other were then applied in order to examine the reliability and the degree of intersubjectivity of the character judgements obtained. Here the investigation also struck on some fundamental problems and research possibilities with regard to the semantical analysis of separate statements on personality. For every statement a *matrix* can be constructed, containing n times m figures, indicating the degree of applicability which each of the (n) judges considers correct with regard to each of the (m) subjects. These "matrices of applicability", if studied systematically, can, in the writer's opinion, develop to an important tool for an empirical semantical analysis of characterological concepts.

Many different research procedures will thus arise spontaneously, if only the initial difficulties of designing a method of statement analysis are overcome. Apart from the differences in method the analogy between Cronbach's study and the writer's is striking. "Validation research should hold the spotlight in the field of personality for the next several years.... We now have projective techniques, situational tests, and numerous other new tools which purport to describe personality; there lies ahead a large chore of validation to define the limits of dependability of each technique. There must be methods of validation capable of throwing out the superficially valid diagnoses that are so general they seem to fit any case, and the confusing pseudo-explanations that describe the person in technical jargon which conveys no real meaning." (5, p. 373). These statements from Cronbach's study could readily be inserted in the writer's 1947 article (7); they would fit perfectly.

Validation research can, however, only be fruitful, if the fundamental problems of falsification, direct and indirect, are intensely studied. Psychologists should — according to the empirical scientists attitude — incessantly seek for falsifiable ways of verbal expression with regard to personality. As directly falsifiable predictions will always remain rather scarce, the main objects for the future are as stated above: to improve our translating techniques from intrinsic to behaviour language, to develop refined criteria for relativated falsification (and verification), and to design, and to try out, improved methods for indirect validation.

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RAPPORTS SPATIAUX DE QUELQUES PHENOMENES PSYCHIQUES

par

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Guidé par certaines expériences acquises au cours de la psychanalyse des malades je suis arrivé, après quelques réflexions théoriques, à supposer qu'il existe des liens beaucoup plus étroits qu'on ne croirait entre la conception que nous avons de l'espace et d'autres phénomènes de notre vie psychique, phénomènes qui, à première vue, n'ont aucun rapport à la conception de l'espace. Les attaches affectives de notre conception de l'espace sont connues. Le psychologue Stern, le philosophe Cassirer, Schilder qui était aliéniste et psychanaliste, se rendaient compte de leur existence comme je l'ai exposé d'une manière plus détaillée dans mon étude sur Bolyai. Dans sa psychologie du développement, Werner souligne que dans l'expérience primitive l'espace est pour le sujet beaucoup plus immédiat, plus affectif qu'objectif et beaucoup plus concret qu'abstrait. D'après lui, l'espace est un attribut constant de l'existence personnelle primitive, il est égocentrique, antropomorphe, physionomique et dynamique. Schilder s'est occupé des modifications pathologiques de la conception de l'espace. Il constate que l'espace n'est pas une donnée indépendante de notre âme, mais qu'il est plutôt en relation étroite avec nos instincts, nos désirs, nos impulsions et nos actions. Les neurotiques éprouvent des changements dans leur sentiment de l'espace et ces changements sont en connexion avec les relations qui les lient aux personnes de leur entourage. La qualité de l'espace marquait par exemple à un malade qui souffrait de phobies la distance qui le séparait de sa mère. Parfois il sentait et percevait l'espace comme infiniment agrandi: son inconscient représentait ainsi la distance incommensurable qui était entre sa mère et lui. La perception de l'espace est selon Schilder un fonctionnement qui dépend de la fonction du libido chez l'individu. Laubenthal, dans son article de 1938, se réfère à Binswanger qui

parle des troubles de l'espace affectif et du déplacement des accents de signification. Tous les auteurs cités établissent des relations entre la perception du temps et la manière dont nous concevons la réalité, vu que notre moi magique et mythique perçoit l'espace autrement que notre moi lié à la réalité de la causalité physique.

Le complément que je voudrais apporter diffère sur deux points des théories citées plus haut. Premièrement, ce n'est pas l'action de la vie psychique sur la perception de l'espace que je cherche à éclairer, mais plutôt l'action de la perception et du sentiment de l'espace sur les phénomènes psychiques, ou, de préférence, l'origine commune de ces deux actions; ensuite, je ne conçois pas l'espace comme géométriquement toujours identique. Suivant les indications de la topologie géométrique, de l'analysis situs, je voudrais examiner l'action des espaces *qualitivement* différents sur la vie psychique et leur origine commune avec les phénomènes psychiques.

Un peu d'attention nous permettra de franchir les difficultés que le sujet nous oppose. Notre espace visuel est de trois dimensions. Toutefois, ce n'est pas seulement cela qui lui donne le caractère d'espace euclidien. Il peut y avoir beaucoup d'espaces de trois dimensions. Il y a exactement trois espaces de trois dimensions qui permettent aux objets de se mouvoir sur un trajet limité sans être déformés. Le premier de ces espaces est celui examiné par Euclide. L'autre est l'espace pseudosphérique construit par Bolyai dont la surface peut être représentée sur une surface de trompette ou de selle. Le troisième, introduit par Helmholtz et Riemann, est l'espace sphérique, qui, quant à sa superficie, est représentée sur une surface *sphérique*. L'espace euclidien occupe pour ainsi dire une place intermédiaire entre ces deux derniers. On peut constater, si l'on examine le processus psycho-physiologique de la vue que le sujet s'efforce tout particulièrement de voir l'objet, de n'importe quelle manière qu'il se déplace entre certaines limites spatiales étroites, comme toujours identique à lui-même, c'est à dire sans déformation. Notre espace visuel cherche à devenir un espace *métrique*, pour employer le terme habituel. Mais il n'y parvient pas. Dans la proximité p.e. les mesures s'aggrandissent. La longueur horizontale d'un plan frontal n'est pas identique à sa longueur verticale; ainsi, l'objet se défigure dans la rotation. Les relations de

grandeur seront différentes à la périphérie et au centre, il y aura une contraction centrale (Tschermak). La courbe est souvent droite et est quelquefois courbe. L'angle droit qui se déplace n'a pas l'air d'un angle droit. La rectangularité est le caractère propre de l'espace géometrique euclidien mais non pas de l'espace visuel subjectif. Il manque à ce dernier un système absolu de mesures.

L'homme instruit qui a des connaissances géométriques est particulièrement habitué à la constance des qualités métriques dans les formes géométriques; il est habitué à observer ces qualités métriques. Par contre, comme nous avertit le géomètre D. König, celui qui est moins versé en géometrie sait contempler le monde extérieur sans le falsifier en s'adaptant aux exigences métriques précises. Notre champ visuel est donc un enchaînement de *qualités* et non pas de quantités. Si nous regardons le monde avec les yeux de l'homme sans instruction, nous verrons tout de suite que notre champ visuel ne fait que s'approcher tant bien que mal de l'idéal euclidien sans toutefois le réaliser. *Notre champ visuel est un espace non-euclidien de trois dimensions.* Dans leur étude de 1938, Pötzl et Hoff analysent le champ visuel des malades du centre occipital et ils trouvent que ceux-ci connaissent encore moins l'espace euclidien rigide que les cas normaux; ces malades voient plutôt une quantité de surfaces qui s'enchaînent d'une manière dynamique et fonctionnelle. De ces surfaces, le mathématicien pourrait développer les espaces non-euclidiens déjà connus.

G. Révész, à qui la psychologie doit plus d'une découverte importante, affirme à bon droit, en parlant du rapport de la perception visuelle de l'espace à l'espace euclidien, que les problèmes physiques et mathématiques de l'espace et le problème de la perception doivent être traités séparément. La perception de l'espace non-euclidien n'est pas un problème physique ou mathématique, mais un problème de psychologie. Helmholtz a essayé de présenter l'espace non-euclidien: l'espace pseudosphérique pourrait être vu à l'aide d'un miroir convexe ou à travers d'une lentille convergente très forte, l'espace sphérique à l'aide d'un miroir concave ou d'une lentille divergente. D'après Révész, ni Helmholtz, ni F. Klein, ni Reichenbach n'ont réussi à présenter l'espace non-euclidien à la vue, parce que l'espace non-euclidien qu'ils produisent ne peut être perçu que sous certaines

conditions. (Die Formenwelt des Tastsinnes 1. 1938. 2—31.)

Ce raisonnement exige deux remarques. La première à propos du problème des dimensions. Le champ visuel a en effet trois dimensions, mais dans son développement il doit être près, à l'origine, de l'espace de deux dimensions; tel est d'ailleurs le champ du tact. On pourrait aussi dire, toutefois, que le champ visuel, en tant qu'espace qualitatif, a six dimensions: en haut et en bas, devant et derrière, à gauche et à droite. (Mach.) Nous devons pourtant préférer l'espace de deux dimensions à celui de 3 et de 6 dimensions, parce qu'il se rattache à la préférence donnée aux extrêmes, préférence qui se fait suivant les lois de l'évolution. Cette fonction psychique primitive caractérise certains processus primaires que j'ai décrits il y a plusieurs années.

Cette fonction joue un certain rôle dans les maladies ou il y a des troubles organiques de la perception spatiale (le malade qui souffre d'agnosie optique suit des yeux les lignes de délimitation des figures; Scheller-Seidemann, 1931.) Dans ces cas-là, le malade s'attache sans aucune cause apparente aux bords des choses. Or, le plan de deux dimensions est la limite, le bord du phénomène spatial de trois dimensions. La préférence donnée, sous certains aspects, à la perception de deux dimensions — en même temps qu'elle simplifie la théorie — est pleinement justifiée du point de vue de l'évolution psychique, et cette conception s'insère facilement dans celle d'un espace de trois dimensions, euclidien même, si l'on veut. Les lémurs, ces primates inférieurs, n'ont pas de vue stéréoscopique, c'est à dire il ne voient presque que des surfaces. Le manque de développement de la partie occipitale de leur substance grise corticale, aussi bien que le pourcentage bas de leur croisement oculomoteur (0.38, à côté de 0, 86 chez le chimpanzé et 0.90 chez l'homme), est conforme à ces faits.

La seconde remarque que j'ai à faire se rapporte à la critique de Révész que je voudrais rendre plus conséquente. Il est bien vrai, que toutes les présentations visuelles connues jusqu'ici de surfaces non-euclidiennes font usage de notre champ visuel de trois dimensions, même si elles veulent présenter à l'aide d'un système de lentilles, suivant la méthode de Helmholtz, la transformation de l'espace de trois dimensions; il faut pourtant remarquer que ce champ visuel, comme je l'ai expliqué plus haut, n'est pas non plus euclidien, bien au contraire — et Révész a

omis de le souligner — c'est dans ce champ visuel non-euclidien que nous essayons de voir l'déal euclidien lui-même projeté sur une surface. En réalité il n'y a donc pas de différence entre la représentation figurée des espaces mathématiques non-euclidiens et l'espace mathématique euclidien.

Enfin, nous ne devons pas commettre l'erreur de concevoir la perception comme une fonction isolée. Nous savons jusqu'à quel degré la perception de l'espace est mêlé à des éléments de mouvement. Le mouvement, l'action d'arriver à un but jouent un rôle dans la représentation helmholtzienne des espaces non-euclidiens: dans l'espace pseudosphérique, les mouvements arrivent à leur but lentement, dans l'espace sphérique au contraire rapidement. Mais ce n'est pas le seul aspect des rapports qui existent entre mouvement et perception de l'espace: d'après Schilder-Martin, la perception de l'espace n'intervient dans les mouvements qu'à leur commencement, après, elle en devient indépendante; (Ainsi elle n'a qu'un rôle très limité dans la résultat final des mouvements qui se dirigent vers un but précis.)

La branche spéciale de la géométrie qui s'occupe non pas du caractère métrique de l'espace mais des phénomènes qualitatifs qui sont indépendants des circonstances métriques, s'appelle topologie ou «analysis situs». Un plan formé par une bande de caoutchouc p.e. présente la même surface avant ou après être pliée ou étirée. Une simple dilatation ne nous fait pas changer d'espace topologique. Lorsque l'espace se dilate simplement — ce qu'il fait toujours au commencement de la vision — ou qu'il se rétrécit — ce qu'il fait toujours à la fin — (c'est le mouvement de Kofka) nous rencontrons toujours, du point de vue topologique, le même espace non-métrique. Le petit enfant néglige souvent les relations métriques — comme le prouvent les observations faites par Mlle Hedvige Kéri dans l'institut de M. Mérei. D'après l'opinion de l'enfant, on peut p.e. faire entrer la poupée par la porte d'une petite maison malgré la disproportion métrique. L'enfant, jusqu'à un certain âge, perçoit d'une manière *topologique et non pas métrique.*

Nous voyons au premier regard que la surface sphérique est fort comprimée. Si nous appliquons étroitement une feuille de papier sur une sphère ou sur une surface de trompette, nous pouvons observer tout de suite la dilatation ou le rétrécissement:

sur la trompette le papier se déchire tandis que sur la sphère il se chiffonne. Nous pouvons encore observer que sur une sphère qui se meut, des points qui paraissent éloignés s'approchent beaucoup plus vite de nous que sur un plan droit euclidien à cause du rétrécissement de la surface; sur une forme pseudo-sphérique à surface de trompette ou de selle au contraire, des points rapprochés de nous ne s'approchent que beaucoup plus lentement, à cause de la dilatation de la surface. En même temps, c'est ce qui caractérise topologiquement les espaces métriques non-euclidiens. La surface unilatérale, comme p.e. la bande tordue de Möbius est un phénomène topologique très intéressant. Sur une surface pareille, il n'y a pas d'orientation possible. C'est pour cette raison que Kerékjártó (Vorlesungen über Topologie, 1923 Berlin) l'appelle une surface sans orientation. Sur cette surface, le mouvement dirigé, la direction conforme ou contraire à la marche des aiguilles sur le cadran n'a pas de caractère constant. Après avoir parcouru un certain trajet et après être retourné au point de départ, le mouvement peut prendre une direction contraire. (Souvent, dans l'espace à trois dimensions, les surfaces unilatérales ne sont pas réalisables sans points multiples, c'est à dire sans des points qui appartiennent à la fois à plusieurs surfaces dites élémentaires.)

Posons le problème d'une manière plus précise: est-ce que des surfaces pareilles, des espaces de 2 ou 3 dimensions existent en effet psychologiquement, peuvent-elles être perçues? Je tiens à souligner de nouveau qu'il y a des éléments non-euclidiens dans notre champ visuel perçu; on peut même dire que l'espace euclidien pur n'est qu'une abstraction mathématique. Nous trouvons, en même temps, trois phénomènes qui nous avertissent du fait qu'il y a en nous des espaces topologiquement différents de l'espace euclidien, espaces certainement primitifs du point de vue de leur développement.

Nous en sommes avertis d'une part par le phénomène des illusions optico-géométriques. Nous y retrouvons l'espace dilaté et rétréci, l'espace qui rompt les cadres métriques, la torsion de l'espace, la ligne droite devenue courbe. Il y a peu de temps que mon attention fut attirée sur le deuxième groupe de phénomènes. Quelques uns de nos artistes qui se nomment eux-mêmes «artistes abstraits» ont organisé une exposition de tableaux. Nous y avons trouvé des tableaux un peu étranges. Beaucoup de

peintures semblaient avoir une vraie valeur esthétique. Mais ne voulant pas m'arroger la compétence du critique d'art j'ai contemplé ces tableaux en psychologue qui sait que «l'artiste abstrait» cherche honnêtement à nous parler des tréfonds de son âme et qu'il veut trouver un contact avec les couches primitives de l'âme du spectateur. Eh bien, il y a avait plusieurs motifs de ligne que j'ai trouvés indentiques non seulement sur plusieurs tableaux d'un même artiste mais aussi sur les peintures d'artistes différents. On y trouvait p.e. des allusions à la surface tordue de Möbius et pour ainsi dire l'expérience de la torsion; on y apercevait la forme de selle et de trompette, aussi bien que les figures de rayon si bien connues des illusions optico-géométriques. On y voyait parmi les oeuvres des sculpteurs le torus, dont nous n'avons pas encore parlé, — et toutes ces figures apparaissaient dans un nombre qui égalait presque celui de leur apparition sur les images du département de mathématique à la dernière exposition mondiale de Paris. Kállai E. écrit dans l'introduction du prospectus de l'exposition que le spectateur doit apercevoir sur ces toiles les signes exprimant des sentiments très bien définis, des instincts qui débordent du subconscient, des passions; il doit apercevoir des gestes picturaux dirigés immédiatement par l'élan du tempérament. En parlant des peintures de Lossonczy il affirme que ceux-ci évoquent et symbolisent des profondeurs cachées, les pressentiments cosmisques de l'âme. Les oeuvres de ces peintres m'ont convaincu qu'il existe en nous des espaces et des surfaces topologiques primitifs, mais que les exigences métriques les empêchent de devenir conscients. Toutefois, l'expérience artistique est capable de les évoquer sur le plan euclidien, tout comme les autres données du subconscient, p.e. le complexe d'Oedipe.

Voici le troisième phénomène: dans des cas pathologiques, nous rencontrons souvent des espaces qui peuvent être interprêtés comme topologiquement différents de l'espace euclidien. (J'ai déja cité les expériences de Pötzl). Dans les cas d'empoisonnement de mescaline les lignes droites deviennent courbes comme si l'on regardait dans un miroir recourbé et la figure des hommes est déformée comme sur les photographies où les mains et les pieds étaient trop près de l'objectif de l'appareil. On voit les objets comme dans un miroir concave, la chambre s'agrandit, les couloirs deviennent profonds, les objets éloignés s'approchent.

Auersperg (1935) décrit un cas de neurologie, ou l'objet lointain semblait plus lointain encore et l'objet approché encore plus approché. Une tour qui était loin avait l'air incliné. Le malade atteint rapidement les objets qui lui semblent éloignés et il ne s'en étonne pas. «Je suis tout simplement ici, au but» — dit-il. S'il dessine, il dessine les objets approchés très grands et lorsque on lui dit d'apporter une correction il les agrandit encore. Par contre, Hoff-Pötzl décrit un cas d'anisotropie qui se produisit lors d'une maladie du centre occipital: dans ce cas, le malade voit parfois les maisons des deux côtes de la rue non pas parallèles mais divergentes. Si le malade tend sa main vers le mur, celui-ci lui semble un peu concave. Il s'étonne parfois visiblement du fait que sa main tâtonnante touche le mur plus tôt qu'il n'aurait attendu (analogie spatiale sphérique). L'aphasique de Bilschowsky sousestime la distance qui le sépare des objets, souvent en connexion avec micropsie. (analogie spatiale pseudo-sphérique).

Un problème spécial se présente à propos de ce que je viens d'exposer: comment est-il possible que des changements intérieurs (neurophysiologiques-psychologiques) produisent le même effet comme si l'on employait un instrument optique (lentille divergente ou convergente, miroir convexe ou concave). Il serait naturellement absurde de supposer l'emploi d'un instrument pareil. J'essaie de rèsoudre le problème dans ce qui suit. La formule commune des lentilles et des miroirs sphériques est la formule $\frac{1}{o}+\frac{1}{i}=\frac{1}{f}$ (o = distance de l'objet; i = distance de l'image; f = distance du foyer). Le changement intérieur est donc nécessairement en quelque relation avec cette formule, il doit être, d'une manière ou d'autre, homologue à la formule. Si nous prenons, au lieu de la distance de l'image, la grandeur de l'image (gi), nous nous rappelons tout de suite une loi «intérieure» (loi entre certaines limites), celle de l'invariabilité de gi, celle de la constance de l'objet. Suivant cette loi $gi = o.c$ ($c =$ constant). Ainsi, toute perception d'images peut être interprétée comme un grossissement ou une réduction obtenus à l'aide d'un instrument optique dont la f dépend de la o. En faisant les substitutions: $\frac{1}{o}+\frac{1}{o.c}=\frac{1}{f}=\frac{1}{o}\,c'$ (où c' est un constant qui dépend du c). Ainsi, la constance de la grandeur de l'image peut être

décrite *comme si* nous voyions à l'aide d'un instrument optique dont le foyer dépend de o. L'instrument optique «intérieur» dont nous avons parlé plus haut n'est donc pas une acquisition nouvelle, mais elle joue déjà un rôle dans la constance de la grandeur de l'image. Il y a naturellement la différence que lorsque l'objet est constant chaque nouvel o nécessite une nouvelle adaptation intérieure et un nouvel «instrument», tandis que, lors d'un changement «intérieur», l'instrument devient indépendant de l' «o», son emploi sera permanent et général....

Avant d'approcher les aspects proprement psychologiques du problème, nous devons faire, après tant d'explications préalables, encore une réflection théorique.

Quel peut être le lien entre phénomène psychique et espace topologique? Je reconnais ici trois relations.

1. Dans la topologie, c'est la notion de *l'ordre* qui permet de grouper l'analyse des formes. (König). Lorsque nous supposons que nos perceptions peuvent être groupées dans un certain ordre, qu'elles peuvent être rangées consciemment, nous réalisons un projet semblable au but de la topologie. Seulement au lieu de nous occuper de lignes et de surfaces nous nous occupons des qualités de la perception. L'ordre des sensations constitue un problème important de la psychologie de la perception. Il arrivait plusieurs fois que pour disposer les sensations dans un ordre on a choisi un schéma spatial; une rangée de lignes circulaires, une pyramide pour les couleurs p.e. Lorsque on a disposé l'échelle des sons, on a aperçu que c'était un schéma spécial, non-euclidien dans le fond qui correspondait aux exigences des faits: une hélice qui monte en contournant la surface latérale d'un cylindre, comme la ligne dite la plus droite; ou pour l'exprimer d'une manière plus compliquée: la projection de cette ligne sur une surface spéciale (surface Riemann) composée d'un nombre infini de couches (Harburger). Schrödinger, le physicien renommé a développé une idée de Helmholtz concernant la mise en ordre des couleurs et il réussit à les ordonner en employant la géométrie non-euclidienne. Une coordination pareille, étant consciente, ne nous intéressera pas dans la suite; elle est toutefois bonne à prouver qu'il ne s'agit pas de supposition théorique, bien au contraire, que ces raisonnements peuvent être réalisés dans la pratique et que nos perceptions ne sont pas étrangères à l'orde non-

euclidien. Je crois que la psychologie dite topologique inaugurée par Lewin, tendence qui a un rôle de plus en plus grand dans la psychologie américaine de ces dernières années, emploie cette première forme de coordination; elle s'y adapte tout aussi bien que la proposition de Bernfeld qui a pour but de représenter topologiquement les rapports intérieurs et extérieurs de l'âme.

2. Fechner était, je suppose, le premier à introduire dans la psychologie la deuxième méthode de coordination; Freud a repris cette méthode et il l'a développée dans sa psychologie topique, la «metapsychologie». D'après sa théorie, les différentes manifestations de la vie psychique se déroulent à l'intérieur de différents endroits hypothétiques de l'âme et les façons de procéder des activités psychiques sont caractéristique pour les endroits psychiques où ces activités se déroulent. Ces locaux psychiques sont la *surface du moi,* le *fond du moi,* (le «Ça»; en allemand: Es) et le *moi* supérieur. Cette différenciation se base sur le développement psychique et sur les méthodes spéciales de travail, mais il n'y a aucune relation intérieure, structurale, aucune coordination entre le local psychique et la qualité de l'activité psychique.

3. Il y a pourtant une troisième forme de coordination à l'aide de laquelle nous essayons de créer des relations étroites entre qualités spatiales d'une part (ces qualités spatiales pouvant être interprétées suivant les locaux psychiques) et phénomènes psychiques d'autre part. Nous pouvons le faire en découvrant de la congruence, de la ressemblence structurale entre espace caractérisé qualitativement (espace qualitatif, topologique) et phénomène psychologique spécial. Nous pouvons dire, en employant l'expression qu'emploirait ici Werner et que le psychanaliste Bernfeld recommande, que nous apercevons des traits structuraux-physionomiques communs (j'emploie le mot structure dans le sens de «Gestalt»). *La méthode de coordination qui met en relief la congruence structurale et physionomique entre la qualité spatiale et le phénomène psychique* fait le sujet principal de cette conférence.

Le phénomène psychique peut avoir trait, par son essence même, à la conception de l'espace, comme par exemple quand il s'agit d'imaginer notre moi corporel, le «Schéma du corps»; dans d'autres cas on ne voit au premier abord aucun rapport pareil, comme quand on parle de la disposition d'esprit ou de

la vie sentimentale. Toutefois, même dans ces cas-là, les mouvements expressifs nous rappellent les relations spatiales.

Nous allons envisager trois phénomènes psychiques: l'orientation droite-gauche, le cas de dépression et de manie et la disposition sadiste et masochiste. Nous chercherons donc à établir l'espace métapsychologique du sujet capable et incapable de s'orienter en direction droite-gauche; celui du sujet qui subit des changements d'humeur et celui du sadiste et du masochiste.

1. L'orientation droite-gauche de la direction exige un espace où cette orientation soit possible, les troubles de l'orientation droite-gauche au contraire peuvent très bien être représentés sur la bande tordue unilatérale de Möbius. Or nous savons que seul l'enfant d'un certain âge est capable de distinguer sans faute la droite de la gauche et qu'il y a une période de transition pendant laquelle il croit que le côté droit, opposé à sa gauche à lui, d'un homme qui est en face de lui est également un côté gauche. (D'après Piaget cela dure jusqu'à l'âge de 8 ans.) Voici ce que je suppose être le fondement de ces deux phénomènes: la surface tordue est plus simple pour l'enfant, le fait que quelqu'un est placé vis-à-vis de lui ravive la surface tordue dont il vient de se libérer; or, l'enfant a peur que le tour de 180 degrés de la personne qui est en face de lui ne le rejette dans la perception spatiale qu'il vient de quitter et qu'il ne perde ainsi son orientation. Le caractère plus naturel, plus primitif de la surface tordue a ses racines dans le fait psychologique que la ligne courbe est plus simple que la ligne droite; d'ailleurs les espaces et les surfaces qui ont trait aux instincts biologiques sont tous recourbés, ils ressemblent à une spirale dans leur courbature multiple. Les surfaces qui représentent l'espace sphérique et pseudosphérique font également ressortir une ligne *courbe* régulière: le cercle. La torsion qui rend l'orientation impossible est un cas de la courbure. Dans un cas décrit par Schilder, la côté malade, dans le schéma du corps, fut en effet perçu comme ayant deux dimensions au lieu de trois. La torsion dans le schéma du corps peut correspondre à une torsion des extrémités. (En dehors de l'incertitude dans l'orientation droite-gauche, la désignation de plusieurs points pour marquer le lieu d'une seule excitation sensorielle correspond aussi à la conception d'une surface unilatérale à torsions multiples.) La coordination structurale et physionomique s'est faite dans ce cas-là entre la surface tordue

unilatérale de Möbius et l'espace métapsychologique de la personne troublée dans son orientation droite-gauche.

2. Essayons maintenant de saisir l'essence de l'affectivité déprimée et hypomane. Binswanger caractérise ces deux cas de point de vue phénomenologique. D'après lui, le malade euphorique a l'impression de se mouvoir dans un espace dilaté; dans cet espace, les choses sont plus près de lui, il les atteint vite et facilement de sa main, mais même les choses approchées lui échappent. Ce caractère consiste dans la facilité de la déviation, ce qui équivaut à la difficulté de saisir les choses. Les personnes hypomanes ont un grand train de vie, ils parlent à haute voix. Cette dilatation particulière de l'espace explique qu'une feuille de papier qui nous paraît grande lui paraît petite, qu'un plan d'action sociale qui est vaste pour nous, lui semble court et simple; une possibilité extrêmement vaque à notre avis est très forte pour lui. L'existence du déprimé se déroule dans un cadre de caractère exactement opposé. Il vit modestement, il se recroqueville volontiers et les grandes distances, les pièces vastes le font souffrir. Il faut encore ajouter que le caractère vif du maniaque s'explique par l'ampleur de son espace; son espace lui concède toutes les possibilités; sa manière de voir les choses l'assure qu' aucun mal ne peut lui arriver, parce que les malheurs sont loin, il a d'ailleurs la certitude de pouvoir obtenir tout ce qui est bon et de pouvoir chasser tout ce qui est mauvais pour lui. La pusillanimité au contraire du déprimé est la suite de ce qu'il considère le mal lointain comme une menace imminente, tandis que sa propre défense motrice est paralysée et qu'il est incapable de penser à un bien éloigné. Nous voyons de ce qui précède le caractère des surfaces sphériques et pseudosphériques apparu dans le moi métapsychologique. Toutefois nous pouvons aussi entrevoir la possibilité de changements brusques d'un état d'âme dans l'autre: le plaisir lui-même est rendu plus difficile dans l'espace et le sentiment de l'inaccessibilité peut s'emparer de la personne hypomane aussi bien que de la personne encline à la dépression, même pendant une activité exempte de dépression. J'avais p.e. un cas dans lequel, pendant une activité très vive qui se déroulait en public, une subite sensation de froid et de rétraction corporelle introduisit le sentiment de dépression qui remplaça ainsi l'attitude active. Chez les personnes enclines à la dépression la peur d'une mort très

proche, aussi proche que les autres malheurs qu'ils prévoient, aboutit à une tendence au suicide. L'éjaculation précoce est une forme spéciale des évènements arrivés trop rapidement, avant le terme, et elle est une des manifestations de la dépression non pas comme cause mais comme effet.

Nous pouvons également répondre à la question pourqoi ces deux états d'âme pathologiques se manifestent dans la forme des deux espaces métriques opposés en tant que cas extrêmes? Dans la manie et dans la dépression, le moi supérieur, de sa part, tombe aussi malade. Dans la dépression, le sévère moi supérieur a une activité trop intense, dans le cas du hypomane, le moi essaie de s' en libérer; mais dans tous les deux cas, la direction est dans les mains du moi supérieur aussi longtemps que la marche des pensées est encore réglée, ce qu'elle est même dans la manie pure. Le moi supérieur cherche à maintenir en vigueur les règles de la vie, les commandements métriques, constants, éthiques, reçus des parents, en somme la mesure des choses.

Derrière les états d'âme maniaques et déprimés il n'y a pas seulement la lutte du moi supérieur et du moi, mais aussi la manifestation plus forte que de coutume des instincts masochistes et sadistes. Les rapports spatiaux physionomiques de ces deux instincts même pris indépendamment de l'état déprimé ou euphorique, sont très visibles.

3. Tandis que l'amour équivaut à un espace dilaté qui comprend la personne aimée aussi et que l'agression est la simple dilatation de notre moi, un des buts de l'instinct sadiste est la dilatation de l'espace personnel au détriment de celui d'un autre. (D'ouvrir le ventre de l'autre, de faire jaillir le sang de son corps, d'écarter ses jambes par force, de vaincre la résistence opposée par la pudeur et d'une manière plus abstraite, l'extension de pouvoir sont autant d'actions qui tendent vers ce but. Mon propre espace sera plus vaste; mais en même temps, si l'on serre la gorge de l'autre si on le ligote, si on écrase son corps axec les pieds, si on lui fait manger les vomissements ou l'excrément, pour lui, le résultat sera contraire: on réalise le but de diminuer son espace autant que possible.) Un malade de Sadger avait des imaginations sadistes: suivant son imagination, il fallait ligoter la femme. Cela pouvait être réduit à l'impression de son enfance d'avoir vu sa mère lacer son corset, c'est à dire se comprimer. La douleur des autres rétrécit également

leur espace puisque elle a pour résultat une contraction parfois spasmodique du corps. Lorsque le sadiste serre la gorge de sa victime il lui soustrait l'air qui est présent partout; ainsi la victime, dans son espace rétréci, se place sous les conditions de l'espace pseudosphérique où il y a impossibilité d'atteindre les choses; en même temps la mort qui doit venir tôt ou tard se rapproche subitement comme accélérée par l'espace sphérique. (D'après Sadger, le plaisir relativement innocent qu'on trouve dans la strangulation aboutit à l'assassinat sadique).

Un de mes malades, dans sa puberté, imaginait des pendaisons et ces imaginations étaient accompagnées de sentiments de volupté. Lorsqu'on tuait un poulet cela l'excitait déjà préalablement et lorsqu'on coupait la gorge du poulet il arrivait à une satisfaction effective. Dans son jeune âge il faillit étrangler son petit frère dans un accès de jalousie. Plus tard il eut des difficultés pendant qu'il mangeait: il avait le sentiment que les aliments, la soupe même, lui restaient dans la gorge et qu'il devait étouffer. A part cela, il avait le sentiment d'une flatulence dans le ventre. Dans sa conduite, il prit des manières arrogantes. Il avait des crises de dépression: dans ces cas-là, les objets se gonflaient, son père avec qui il luttait dans ses imaginations subconscientes devenait un homme aux épaules très larges et il croyait dans la rue ne jamais arriver jusqu'au bout du chemin. Le monde extérieur lui est devenu pseudosphérique.

Une excitation peut serrer la gorge et elle peut inspirer le sentiment d'une contraction de l'estomac. La vue de scènes sadistes peut transformer ce sentiment chez le spectateur juvénil: plusieurs de mes malades me disaient qu'ils se sentaient près de l'évanouissement, ils avaient des dégoûts et des nausées, des pressions sur les yeux etc. Ces mêmes sensations étaient évoquées chez une de mes malades par un collier trop étroit, c'est à dire une contraction de la gorge. Lorsque, dans un âge déjà adulte, on lui pressait un oeil, la crainte de la mort l'envahissait. L'agression extérieure manifestée par strangulation et dirigée contre d'autres personnes se transpose, comme contraction de l'espace, sur son oeil et déclenche l'action du réflexe. Une autre femme éprouvait la même sensation, lorsqu'elle était petite fille, chez le coiffeur, c'est à dire à côté d'un homme qui agitait des ciseaux.

Le pied dilate l'espace; il dilate notre espace lorsque nous

marchons mais en même temps il comprime, il serre. Le soulier peut prendre sur lui ce rôle de comprimer. Une trace de pied, chez une de mes malades (la même qui me faisait part de la réaction de dégoût décrite plus haut) fut liée, par association, pendant la lecture de Robinson, au cannibalisme. Elle lisait souvent ce livre, mais lorsqu'elle arrivait à l'endroit où il est parlé de cette trace de pied, elle devait toujours interrompre la lecture pour un certain temps. Elle était plusieurs fois tentée, pendant sa vie, par le végétarisme; ce qui est bien compréhensible, parceque la vue de scènes sadistes lui inspirait, en dehors des sentiments décrits plus haut, des sensations génitales.

L'organe principal pour exécuter des actes sadistes est la main et c'est en même temps elle qui contribue le plus à la formation de notre espace. Elle est aussi en connexion étroite avec la bouche, qui joue un rôle considérable dans la formation de l'espace primitif et avec les pieds, la bouche et les pieds étant également des organes des actes de sadisme. Il y a d'ailleurs une relation entre les mains et les yeux aussi, vu le rôle des yeux comme organes de former notre espace. L'instinct partial „anal", qui est en contact avec le sadisme, a aussi sa part dans la formation de l'espace; il paraît que l'art du statuaire est une sublimation de l'instinct anal.

Les mesures sadistes qu'on a prises contre les juifs avaient aussi trait à la dilatation de l'espace de ceux qui ont décrété ces mesures, à la prétention d'envahir toute la terre, et d'autre part au rétrécissement de l'espace des juifs (p.e. la concentration des juifs dans un district des villes avant les déportations, l'empilement d'un grand nombre de personnes dans un wagon etc.)

Du point de vue clinique, une forte tendence sadiste peut s'ajouter à la manie de vagabondage qui accompagne parfois l'épilepsie (Kronfeld), à la dépression (Krafft-Ebing) à des anomalies de la substance cérébrale frontale, temporale et occipitale (le cas be Lasègue). Tous ces cas permettent de supposer une connexion à la perception spatiale dont il est question ici.

La relation étroite qui existe entre le masochisme et la dépression, aussi bien que le caractère du masochisme comme opposé au sadisme, explique suffisamment le rôle que le rétrécissement de l'espace peut avoir dans le masochisme. Cette tendence peut souvent être ramenée au fait que le malade était emmailloté trop

étroitement lorsqu'il était nourrisson. La relation est spécialement accentuée s'il s'agit de la vue de scènes masochistes. Le croyant attend des miracles de la part de la personne qui se tourmente, de la part de la personne sainte. Or le miracle n'est que l'apparition soudaine d'un bien lointain, qui était impossible à atteindre. Par le fait que le rétrécissement de l'espace se produit chez une autre personne, l'approchement rapide occasionné par l'espace sphérique peut concerner le «bien» aussi, non seulement le mal comme dans les cas de dépression; tout comme dans les phobies l'espace pseudosphérique extérieur concerne le mal. Le rétrécissement spatial du masochisme explique sa relation bien connue au fétichisme de souliers. (D'après Kronfeld, le masochisme a souvent ses racines dans des tendences à l'évanouissement, dont nous venons de souligner le caractère rétrécissant).

Nous trouvons ainsi que le sadisme qui est souvent déclenché par une satisfaction forcée au lieu d'une satisfaction génitale impossible (Fenichel) ou par une saison chaude, par une situation sociale insupportable, par une disette de logement (Kronfeld), montre, tout comme son opposé le masochisme, des traits spatiaux sphériques ou pseudosphériques; il est vrai que l'ordonnance de ces traits n'est pas aussi exacte que dans l'état maniaque ou déprimé, mais plus chaotique. Ceci correspond au fait qu'il n'y a pas de moi supérieur qui pourrait créer de l'ordre et qui ait de la suprématie dans la direction. En décrivant le sadisme et le masochisme nous avons pris en considération non seulement la description phénoménologique mais aussi les tendences qui concernaient l'espace extérieur; cela rend nos constatations qui se rapportent à l'espace métapsychologique du sujet plus compliquées.

Il ne sera peut être pas surprenant si nous constatons que la torsion est également une partie importante de l'action sadiste. De tordre le cou ou les membres d'une personne est une manifestation agressive sadiste. (La torsion de cou est un mouvement primitif). Le détournement de la tête est connu comme action érotique. La peur d'être saisi par le cou est une phobie enfantine bien connue. Cette peur, si nous faisons abstraction de son sens transposé, concerne la strangulation ou la distortion du cou. Physionomiquement la torsion est congruente à l'espace tordue que nous connaissons comme l'espace des troubles d'orientation.

D'après cela, le sadisme et le masochisme ont une relation aux espaces tordus qui rendent l'orientation impossible, cette relation existe par des raisons topologiques spatiales.

Quoique la conception psychologique phénoménologique et descriptive aient leur raison d'être à elles seules, il n'aurait pas été intéressant de nous occuper de toutes ces relations spatiales si elles n'ouvraient pas des perspectives plus larges. En effet, nous pouvons apercevoir que ce qui précède contribue à l'extension de nos connaissances psychologiques et neurologiques. Nous pouvons aisément nous rendre compte de la contribution à la psychologie. Chez les personnes empoisonnées de mescaline nous rencontrons souvent des changements dans l'espace visuel, accompagnés parfois de troubles dans la perception du chaud et du froid. Ces changements sont très souvent coordonnés à des états d'âme euphoriques, rarement aussi à un état d'âme déprimé. L'espace fait plaisir à la personne empoisonnée de mescaline et il y a des témoignages suivant lesquels c'est le changement optique qui a inspiré la bonne humeur. Nous savons d'ailleurs que toute désorientation, spécialement la difficulté dans l'orientation droite-gauche peut occasionner très vite des manifestations agressives sadistes. Nous voyons maintenant pourquoi cela est possible: l'instinct sadiste lui-même comprend une recherche d'orientation spatiale, un retour à des formes d'orientation primitives et parfois il cherche à demeurer dans ces formes primitives; il peut même congruer à une surface tordue. Les changements spatiaux mériteraient donc d'être considérés comme des phénomènes fondamentaux qui ont une influence sur la vie affective. Ils ont leur place parmi les phénomènes qui forment et déterminent les phénomènes psychiques.

Les troubles dans l'orientation de direction droite-gauche peuvent être considérés non seulement de points de vue psychologique, physionomique et topologique. Lorsque ces troubles se présentent dans le schéma spatial du propre corps ou dans le monde extérieur ou dans les deux à la fois, lorsqu'ils se présentent par des raisons organiques et neurologiques et ils rendent l'orientation difficile ou impossible, l'expérience clinique dit que c'est le lobe occipito-pariétal qui en est responsable. Du point de vue de la psychologie spatiale c'est l'apparition de la surface tordue de Möbius qui explique ce phénomène. Voici donc la question qui se pose très nettement: *quelle corrélation peut*

on supposer entre l'expérience neurologique et la théorie psychologique?

La rotation du corps autour d'un axe longitudinal produit d'après Schilder des symptômes occipitaux et pariétaux à cause des troubles du méchanisme cérébral pariétal. (Sur une influence vestibulaire nous rencontrerons multiplication, rétraction, interpénétration des images vues). Une lésion occipitale et pariétale augmente, d'après Schilder, Hoff et Gerstmann l'impulsion rotatrice du corps autour de son axe horizontal. Le malade de Niesl v. Mayendorf, souffrant de cécité psychique voyait une épingle à cheveux comme un tire-bouchon, c'est à dire qu'il voyait une ligne tordue au lieu d'une droite. (On peut considérer comme la cause de ce changement aussi un miroir déformant imaginaire). Après l'excitation sensorielle causée par une impulsion rotatoire, Mayrhofer observait un détournement, Auersperg une déclinaison d'un certain angle dans la sphère optique, Pötzl l'anisotropie du centre occipital et une déviation des murs de la chambre de 135 degrés.

L'endroit pariétal-occipital supprime, d'après Hoff, les impulsions rotatoires primaires des yeux. Engerth a observé que dans un cas pariétal le malade voyait une projection sur une surface au lieu de l'espace, et cela était accompagné de troubles dans l'orientation droite-gauche. D'après ce que nous venons d'exposer, des anomalies organiques ou fonctionnelles de la substance corticale occipitale et pariétale produit des symptômes qui peuvent être interprétées par une déviation de l'espace et qui font changer l'espace en surface. La substance corticale occipito-pariétale a une connexion visible aux impulsions rotatoires. La qualité topologique de la bande de Möbius est donc le résultat d'une fonction primitive et refoulée de la substance corticale occipito-pariétale. Cette localisation relie la théorie psychologique à la théorie neurologique.

Les manifestations sadistes et masochistes des instincts peuvent facilement être influencées par le diencéphale où nous trouvons les endroits qui règlent le ton musculaire, la vie végétative et l'effectivité et qui signalent les douleurs intérieures. Le chien thalamique mord p.e. férocement, le chat thalamique souffle très fort s'ils sont atteints d'excitations douloureuses. Le malade qui vient de se rétablir d'encéphalite peut aussi montrer des symptômes sadistes et agressifs pareils. Le lémur qui a une vue

astéréoscopique possède une cervelle carnivore, c'est à dire d'un type plus agressif. Le macaque opéré bitemporalement sera atteint de cécité psychique. Les influences affectives se perdent, il veut tout toucher de sa bouche, il accepte de l'aliment animal. (Hooton) A côté d'une sexualité accentuée et des perversions (activité homosexuelle violente) son aggressivité semble augmenter, quoiqu'il ne devienne pas furieux.

Le lieu céphale est considéré egalement comme responsable pour les états de délire et d'hypomanie. Le corps mamillaire et ses communications sont désignés dans le diagnostic topique de l'hypomanie. Une suppression des fonctions de la partie antérieure du sous-thalame peut produire un état d'âme déprimé, l'excitation de la même partie des états maniaques. (Förster–Gagel). Une tumeur de l'endroit du corps quadrigem. peut aussi être accompagnée d'un état tout classique. Ce qui est intéressant pour nous, c'est que beaucoup de psychoses qui ressemblent à l' hypomanie et qui sont localisées cérébralement sont accompagnées de symptômes spatiaux et visuels. Déjà dans le beau cas communiqué par Korányi il y avait des idées de jalousie visuelle, la visualité étant d'ailleurs une qualité générale des imaginations de la jalousie. Dans de cas de Myers (hypomanie sous-thalamique) une forte tendance de prise de direction vers le côté gauche est très remarquable parmi les symptômes. Dans un cas maniaque de Pötzl, où il y avait une tumeur de diencéphale, on peut trouver de l'agnosie optique simultanée. Sur 7 cas maniaques localisés de Foerster–Geges, dans six cas on remarquait de l'hemianopsie ou d'autres anomalies visuelles.

Notre attention est donc attirée par la physionomie du sadisme et du masochisme et par la physionomie de la dépression circulaire sur le rôle que le diencéphale et le mésencéphale jouent dans la physiologie de la vue. Le thalame et son environnement avait une fonction primitive dans la vue qui fut ensuite abandonnée au cours de l'évolution. Chez les animaux inférieurs, les corp. quadrigem. anter. servent d'organe pour la vue, chez les mammifères inférieurs à des fonctions optiques, chez l'homme et chez le singe à des fonctions de réflexes. Le pulvinar est un relais, d'apres C. Winckler il exerce une influence sur la vision des formes. (Hoff–Pötzl). D'après ce dernier cette influence s'exprime dans l'harmonie de la vue en profondeur. Pötzl (Ueber die Beteiligung des Thalamus am Sehakt) arrive aux résultats

suivants: les plans se recourbent sur une influence du thalame et dans l'aura d'épilepsie influencé par le thalame la partie inférieure de l'arbre qu'on voit recule très loin, ce qui peut être expliqué par une cessation périodique de l'adaptation visuelle. Ceci correspond à un emploi local d'une lentille divergente, c'est à dire, suivant la théorie helmholtzienne, à l'introduction d'une surface (espace) sphérique. Pötzl prouve l'existence d'une action réciproque thalamo–occipitale, il croit même que l'intervention du labyrinthe est aussi possible. (1942). Il attribue un rôle très important à cette influence réciproque dans la formation de l'espace visuel; il suppose que la formation de l'espace dans les images du rêve et du demi-sommeil doit être attribuée à un dérangement de cette action réciproque et il considère le système vestibulaire comme ayant un rôle ordonnateur. Physiologiquement le thalame lui-même est un centre trophique et c'est par là qu'il exerce une action sur les matures parties du cerveau. Le nouveau-né humain peut être considéré comme une organisation du type thalamique. Les systèmes optique et olfactorique reçoivent leurs gaines moelleuses quelques semaines après la naissance aussi bien que les nerfs qui partent du thalame.

Sachant que le diencéphale influence notre affectivité, qu'il est même peut-être le siège de certains instincts, nous devons nous rendre compte de ce que la physionomie de l'affectivité s'exprime parallèlement à des qualités spatiales primitives et que, d'autre part, la formation des espaces libres primitifs se fait à l'aide des mêmes parties du cerveau que la formation des sentiments. Notre affectivité, nos instincts et les formes spatiales sont donc étroitement entremêlés par des raisons neurologiques et organiques, notre affectivité et nos instincts se sont développés ensemble avec ces formes spatiales, surtout avec des qualités spatiales primitives et changeantes. Une régression dans la qualité spatiale doit être étudiée en même temps que la pathologie sentimentale. Cette étude ne peut se faire qu'en se libérant du système des formes euclidiennes, sous la direction de la topologie, en luttant contre la résistance qui se présente dans la perception et dans la théorie. Tous les cas psychopathologiques et psychiatriques doivent être examinés à fond, pour voir leur connexion à des espaces topologiquement qualifiables, pour voir quel est l'espace que le malade désire, quel est celui dans lequel il se déplace, celui qu'il dessine avec prédilection, celui qui fait

partie de sa perception, ceux qu'il n'aperçoit pas même si l'on les montre à plusieurs reprises. On doit examiner la vie cachée des instincts chez les personnes qui souffrent de troubles organiques dans leur perception de l'espace. Ensemble avec ces recherches, on doit spécifier la physionomie spatiale propre aux diverses manifestations affectives. Cela peut être une méthode nouvelle et belle du diagnostic et un nouvau modèle de l'interprétation des phénomènes psychiques.

Résumé: Après avoir rappelé que notre espace visuel comprend des éléments non-euclidiens et que les exigences métriques (de la constance des mesures) offusquent le caractère réel de la perception, après avoir montré que ses éléments se trouvent plus souvent et d'une manière soulignée dans les oeuvres d'arts et dans les cas pathologiques, nous avons énuméré quelques espaces qualitativement différents, connus de la topologie géométrique. Nous avons prouvé que l'espace et le phénomène psychique peuvent être considéres comme ayant trois relations différentes l'un avec l'autre: relation de disposition, relation locale métapsychologique, et, dans la métapsychologie, relation structurale et physionomique. Nous avons examiné, en nous basant sur la relation physionomique. 1. les troubles de l'orientation droite-gauche, 2. l'affectivité hypomanie et déprimée, 3. les activités sadistes et masochistes des instincts, c'est à dire les espaces métapsychologiques qui leur correspondent. Ces espaces sont les suivants: 1. la surface tordue unilatérale de Möbius, 2. la surface (espace) pseudosphérique et sphérique fortement différenciées l'une de l'autre et 3. tous ensemble dans un état chaotique. Tous les trois phénomènes que nous avons examinés peuvent être localisés, dans une certaine mesure, dans le cerveau; en nous basant sur cette localisation nous avons souligné que le lobe occipito-pariétal, le diencéphale produisent des espaces primaires primitifs et qu'ils déterminent en même temps les phénomènes psychiques.

Summary.

It is pointed out that our optical space contains non-Euclidean elements and that the claim of metrical constancy obscures the true characteristics of perception. It is further demonstrated

that such non-Euclidean elements may be present in works of art and under pathological conditions. To explain their nature different spaces, all of them known in geometrical topology, are discussed. Then the relationship of space and psychical phenomena is defined which may be of three kinds, viz. that of order, position (metapsychological) and, within the latter, of a Gestalt-physiognomic character. Physiognomic-spatial relations are then examined. 1. in case of the disturbance of right-left orientation, 2. in case of topological spaces that correspond to mania and depression, and 3. of those that are connected with sadistic-masochistic experiences. These spaces are 1. the twisted unilateral Möbius band, 2. the pseudo-spherical resp. spherical surface in clear distinction, 3. all these together in chaotic disorder. The three phenomena can in some measure be localized in the brain and on these grounds it can be surmised that the parietal and occipital lobe and the middle brain are able to create primary spaces and at the same time determine psychological phenomena.

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THE EFFECT OF LESIONS OF THE BRAIN ON THE "INTELLIGENCE" OF RATS

BY

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During the past three decades various investigators in America, e.g. Lashley, Layman, Ghiselli, Brown and Harrick have been engaged upon the problem of intelligence. The first question to present itself was which cerebral processes are essential for intelligent behaviour. In these investigations the presupposition was taken for granted that in man and in the higher mammalia it is the function of the cerebral cortex to gain insights and to utilize individually acquired experience, in contradistinction to such behaviour, as can be summarized under the heading of reflexes and instincts.

Gaining insights and utilizing individual experiences might stand for the correlate of intelligence. The question is whether intelligence must be regarded as an indivisible function of the activity of the whole of the cerebrum, as was the view held by Flourens, Goltz and Loebe, or whether the intelligence is linked to the cortex cerebri. If the latter is true it still remains an open question whether the cortex as an aggregate of the sensory and motor spheres is the seat of intelligence (in the sense of Von Monakow) or whether there are special cortical fields which are definitely concerned with the intellect and the behaviour.

In our country, too, investigators (ten Cate, Buytendijk et al.) have been working on this problem. They have endeavoured further to analyse the concept of intelligence by tracing the extent to which various experimental animals were disturbed in their behaviour after certain portions of the cerebral cortex had been experimentally injured.

In the experiments to be discussed below I have used rats as experimental animals, as did the investigators referred to above. Rats are very suitable animals for these investigations, as they are active, whilst they are open to a certain communication

with man in their captivity and moreover can be bred in certain strains without disease.

The interpretations of the results of the animal experiments are rather divergent. There are even investigators who dispute the practicability of the method used by us. Buytendijk int. al. is of opinion that not only the injured area drops out in consequence of the heating of nerve cells, but that the cauterization produces cell toxins which give rise to further disorders, whilst moreover functional disorders may arise in the adjacent nerve cells, without histologically demonstrable alterations in the tissue. In his opinion it would be difficult if not impossible to determine the relation between the cortical area that has been removed and the phenomena observed.

We have carried out several investigations in the neuropathologic and psychopathologic laboratory with a view to throwing some light on this matter. We have induced lesions by means of agglutination and afterwards we have carefully noted down these losions in the diagrams drawn up by Lashley et al.

Moreover the whole of the brain was submitted to an histopathological examination after the autopsy with a view to ascertaining the extent to which necrobiosis had made its appearance and the extent to which secondary degeneration had occurred in consequence of the primary lesions.

For the purpose of studying the results we have distinguished for the time being between the healthy rats and the ones presenting only cortical lesions, whereas such rats as had subcortical lesions only are to be studied later on.

Accordingly, the results of these investigations only concern cortical lesions for the time being.

When summarizing Lashley's considerations, who has been the pioneer in this field, we might get the impression that cortical lesions are of little importance in rats in view of the fact that the behaviour of healthy rats is not influenced by cortical lesions.

The apparatus for learning is not localized in the cerebral cortex, according to Lashley, or to put it otherwise, it is impossible to locate any structure or any anatomical mechanism in the cerebral cortex, function of which facilitates insight and learning. Furthermore, it would be immaterial, according to Lashley's investigations where the lesions in the cortex are

induced: from 20 to 40 per cent of the surface of the cerebrum can be removed without having any repercussion on the capacity to learn or on the insight.

Lashley consequently regards the results of his investigations as being contrary to the traditional conceptions on the cortical localization. He admits, it is true, that in various experiments a once acquired behaviour is forgotten after cortical lesions have been induced, but this behaviour is learnt again in the same stretch of time by the animals operated upon. This learning requires subcortical mechanisms and a part of the cerebral cortex, in which it is immaterial according to Lashley which portion is used, and therefore the whole of the cortex is equipotent as far as learning is concerned.

As regards the facts we have likewise found that the simple maze test is learnt and retained after destruction of any third part of the cortex. The more difficult maze tets are not retained any longer if part of the cortex is destroyed, but after that they are learnt again in the same stretch of time. Consequently this would imply that the cortex does have a significance as far as the retention of habits is concerned without being essential, however, for the insight. These experiments also reveal that a certain portion — one half at least — is necessary to gain insight.

The double-terrace-test (vide fig. 1) confronts us with greater difficulties, however. The time required to learn how to fulfil this task is found to be, in the animals operated upon, half of

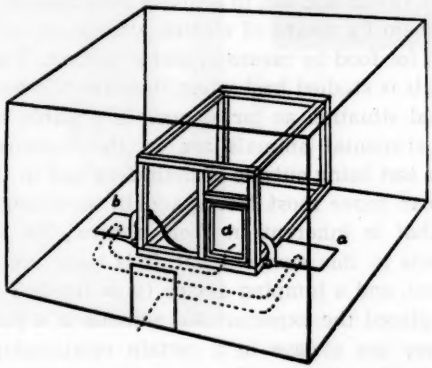

Fig. 1. The double-platform box used in earlier studies. The door (*d*) is opened when the platforms (*a*, *b*) are successively pressed down

the time used by healthy rats. At first sight this means that the cortex exercises an inhibitory action on gaining an insight into the double-terrace-test. A third experiment of the Behaviourists may be mentioned here, viz, the visual discrimination test. In this test as well as in the double-terrace-test a neuromuscular apparatus has to be adjusted, for which an intact cortex is not required. It is fairly well possible, on the other hand, that cortical activity or cortical participation plays a rôle in establishing these habits. In is an established fact, however, that this test, too, is solved without cortical lesions as easily as it is with cortical lesions. This is the more important as with this test it is not a matter of responding to a given stimulus or a series of stimuli but of realizing the relationships between the physical elements in the total situation; thus it is less a matter of observing the separate elements, i.e. in these tests the relationship consciousness called on and this realization of relationships is thought to be a specific factor in the intelligence.

It may be emphatically pointed out here that insects and other brainless animals are known "to realize" relationship when they have to fulfil some task rather than to respond to the absolute intensity of the stimuli, (frame of reference.) Therefore it is not astonishing that the function of the cerebrum is not a precondition for mastering these tests.

We have made some experiments with our animals that differ from the ones made in the school of the Behaviourists, because it is not right, in our opinion, to provoke the action of the animals by rousing them by means of electric stimuli, or by compelling them to look for food by means of starving them. The behaviour of the animals is studied best when they are allowed to remain in their usual situation as far as such is possible in captivity; i.e. the experimental animals are in their environment the leader of the test being outside of their cage and in this environment they can move most freely and in accordance with the behaviour that is inherent in their nature. We have placed certain objects in this environment, it is true, viz. a ladder, a running-board, and a jumping-device (vide fig. 2, 3 and 4), that is, we have placed the experimental animals in a play situation in which they are always in a certain relationship to a test leader. In this play situation they sometimes assumed a position in the jumping-device, from which a different situation could

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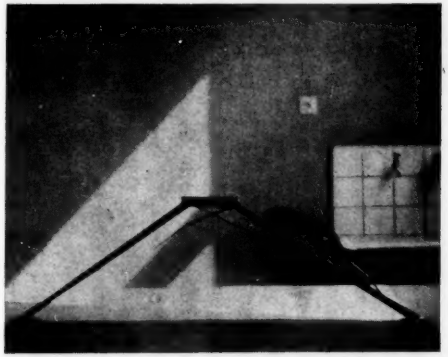

Fig. 2

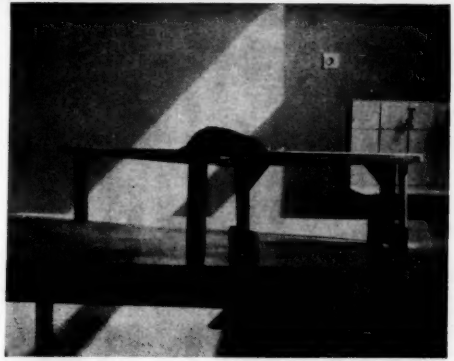

Fig. 3

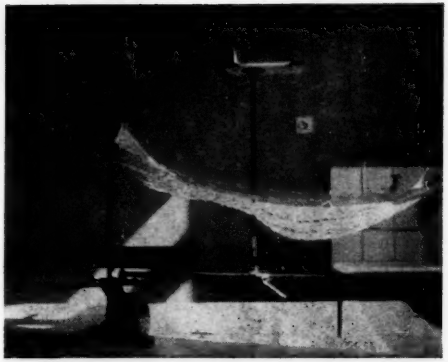

Fig. 4

be sought which is more stabile as compared with the former.

This might be compared with a child playing under the supervision of or at least in the vicinity of a leader who has permitted it to stand on a wobbling board, or it might be compared with the behaviour of a motorcyclist in a somewhat complicated traffic situation. At that moment rat, child and motorcyclist have to look for a different, more stabile position, departing from the position held by them, which is pleasurable for them and which they do without experiencing it as something unpleasant and as something that is forced upon them.

With the aid of cinematographic shots we have recorded the behaviour of the animals. When studying these shots it is impossible at first to ascertain the points in which the animals operated upon distinguish themselves from the healthy ones. The experimental animals are observed to behave like normal ones in spite of the operation performed on their brains. Consequently a statistical elaboration of this case material might suggest that the cortex, or at least one third of it were irrelevant as far as the behaviour and the intelligent command of a situation or a problem is concerned. The question is, however, what is understood by intelligence, and in this instance with our experimental animals, by practical intelligence?

This practical intelligence is somewhere in between instinct and thinking faculty. A common feature of intelligence and instinct is that the former is aimed at a rapid and positive solution of the problems raised by the situations in which we are placed by the circumstances of life. However, the instinct is not preceded by judgment. Furthermore the instinctual action is repeated with an inner necessity as is to be observed in the building of nests and the hunting instinct. Intelligence operates with what is called judgment by us, it contains a conscious factor, there is an insight. Yet is is not a result of the thinking faculty. It is not intellect, nor drawing consistent conclusions from given premises. Intelligence distinguishes itself from the purely intellectual faculties in one given respect: i.e. the relationship to the concrete data. The thinking faculty which is adjusted to formal conditions will often fail when confronted with concrete circumstances (such as a problem of traffic). We are required then to take in a number of simultaneously possible situations, which cannot be grasped purely logically, at least not in the

brief spell of time available. Both in theoretical and in practical intelligence we meet with the accomplishments of the thinking faculty, but they distinguish themselves from the formal conclusions of pure logic. Intelligent behaviour, response, has the character of an inspiration of suddenly knowing, without knowing why. In theoretical as well as in practical intelligence we are concerned with the natural ability of the personality consisting in an inner mobility, a certainty and easiness in the orientation. It is always a matter of solving problems which were not present in experience in this concrete form.

When giving for instance an experimental subject two small boards, one of which can be dovetailed to the other and asking him which is the most stabile position of the latter board when it is set up perpendicularly and may be supported by the former, it is remarkable to observe the divergence of the answers given. For the smaller board is made up of two parts which are linked up by a hinge. A piece of lead is attached to the smaller part. The correct solution of the problem is that the larger part is put into the dovetail there, where it sticks. Not only the correct solution is of importance however, but the way in which this correct solution is secured. A good practical intelligence manifests itself in that the solution makes its appearance as a suddenly arising insight into the often primitive spatial and technical relations in connection with a given instruction. When we are driving a car our practical intelligence is tested every moment, whenever e.g. a tire bursts or a dog suddenly crosses the street, or when we are driving through a crowded thoroughfare. We investigate the way in which and the rate at which the subject acts in a given situation.

Consequently we speak of intelligence when in concrete situations one is required to take in a number of simultaneous events, varying one from the other, which cannot be deduced in a purely logical way, at least not in the time available. Thus it is a suddenly arising insight into the physical-technical relationships, a suddenly arising control of a concrete situation, which control manifests itself firstly in a certainty and easiness in the orientation, secondly in the speed and thirdly in the qualities of the behaviour. When viewing once more the behaviour of the operated rats from this angle it is found that the capacity for adaptation to altered situations has markedly

suffered, that the speed has diminished and that the quality of the behaviour has declined. Moreover we discover in a phenomenological analysis of this behaviour another two very important facts: Firstly the biotonus of the animals having cortical lesions has deteriorated. Their spontaneous movements are slower, less coordinated and have less tonus, in consequence of which the efficiency of the motor system is more disturbed than is that of blind rats. It is as if the intact cortex maintains the motor responsiveness, as if the readiness for action has suffered as a result of cortical lesions, and therefore, when expressing it biologically, the biotonus has certainly decreased in consequence of the cortical lesions.

Secondly it appears that it is not immaterial where the lesions are induced. These investigations correspond with those carried out by the Americans in that unilateral lesions do not markedly affect behaviour, and therefore one might say approximately in accordance with the presupposition advanced by Lashley that the cortex has a vicarious function provided that a lesion is induced on one side only. However, if the lesions are symmetrical, behaviour has distinctly suffered, whilst moreover a very great difference can be observed between defects in the posterior and defects in the anterior third part of the cerebral cortex.

For it has become apparent that the sensory-motor constructions built up on optic stimuli are the ones that have suffered most in the event of lesions of the posterior part of the brain (area striata; area W and M I of Droogleever Fortuyn), whereas the constructions based upon tactile stimuli are most seriously affected when the middle part of the cortex is injured (area N and the caudal part of area F and F 1).

Finally we wish to point out that our investigations were carried out on experimental animals shortly after the operation, in some of them from four to eight weeks after the operation whereas another group was left alone until seven or eight months after the operation. The American investigators are of opinion that the ability to learn again is a faculty correlated with the amount of cerebral matter. Accordingly, the process of learning would take a longer time in extensive lesions since learning is acquired by exercise as a function of mass and time. If our conceptions on intelligence as an ability that is specifically different from learning is correct the time elapsed after the

operation in our investigations will not permit of such a statistical elaboration as is desired by Lashley. Our animals operated upon displayed a behaviour that was in accordance with von Monakow's concept of diaschisis rather than an increased learning following the operation. Experimental animals used when four or five weeks had elapsed after the operation met with great difficulties in the jumping-device test and did not make any progress, whereas after a certain lapse of time they fully commanded the situation as did those experimental animals that were put into the jumping-device for the first time nine months after the operation.

Summarizing, we feel justified in stating that the vicarious capacity manifests itself to a very slight extent in the event of lesions of the cerebral cortex. Secondly, that the intelligence is impaired in general as regards the easiness and the certainty in the orientation, the necessary speed and the qualities of the actions. Consequently the intact cortex seems to be indispensable for an intelligent behaviour, when defining as such a sensory-motor construction adjusted to a constantly changing situation. In the maze-tests and other tests based on reflexes subcortical centres may meet the requirements. Furthermore it became apparent from our investigations that there is undoubtedly a differentiation in the function of the cortex, the anterior part being concerned with the tactile motor behaviour, the posterior part with the optical processes. This does not mean that the area striata does function as it does in man; the rat was found to be unable to see only when the colliculi had been completely removed. Its visual functions were undisturbed when the colliculi had been preserved.

Summarizing we may state that the intelligence of rats cannot be investigated if the tasks to be fulfilled are limited to performing such actions as are linked to the subcortical system, by virtue of their being reflexes or instincts. Therefore we must observe the experimental animal in the situation of life in which it normally finds itself. When doing so we encounter the vital phenomenon that is called intelligence by us in the functioning of the cortex, although this phenomenon is often preformed as an instinct in the subcortical centres. The more "intelligent" manifestations of life will be effected therefore with greater difficulty if the cortex has been impaired.

In concluding it may be of interest to point out that Langley himself modified his views in the end of 1938, when writing that the picture of the restitution of functions following cerebral lesions should be represented much less optimistically than he had imagined fifteen years before.

The fundamental effect of the cerebral lesions expressed by Lashley at the time as a "loss of interest", is not obviated by "time" and "training".

ÜBER URAFFEKTE

von

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Es ist eine eigentümliche Tatsache, dass der Ausdruck Affekt in dem Sinn, wie er in der Psychologie gebraucht wird, nicht in das Englische und Französische übergegangen ist und im Deutschen, das doch geradezu an einer Überfülle von Bezeichnungen für seelische Tatbestände leidet, keine angemessene Übersetzung gefunden hat. Denn wie das englisch-französische *affection* eine zu eingeschränkte, so besitzt das deutsche Angetansein eine zu allgemeine Bedeutung. Am nächsten kämen dem Begriff Affekt noch die Ausdrücke *passion* oder Leidenschaft, aber freilich nur in ihrem ursprünglichen Sinne, der sich im Laufe der Zeit geradezu in sein Gegenteil verkehrt hat, sofern die Bedeutung des blossen Erleidens durch die Bedeutung eines gesteigerten Tätigkeitsdranges ersetzt worden ist. Indessen liegt vielleicht gerade an dieser Stelle die Lösung des Rätsels, denn solange die Psychologie im Bann einer Erkenntnistheorie der *tabula rasa* oder der *camera obscura* stand — die in letzter Linie auf die Erkenntnistheorie des Platonischen Höhlengleichnisses zurückgeht —, konnte ja der gesammte Inhalt des Seelenlebens nur aus Affekten, *passiones* oder *πάθη* bestehen, weil sich der einzige kümmerliche Rest von Tätigkeit, welcher der Seele verblieben war, in der Verknüpfung oder Assoziation der ihr „angetanen" Vorstellungen oder Ideen erschöpfte. Wenn also schliesslich der Inhalt des gesammten Seelenlebens, wie bei Kant, zu einer blossen „Affektion" wurde, so verlor damit der Begriff des Affiziert- oder Angetanseins seinen Eigenwert und damit zugleich seinen Gebrauchswert zur Bezeichnung bestimmter seelischer Funktionen.

Fragt man aber weiterhin, wieso es scheinen konnte, als ob alles Seelenleben in einem blossen Erleiden äusserer oder innerer „Eindrücke" ohne jede Selbsttätigkeit bestehe, so liegt der Grund offenbar darin, dass die Psychologie seit dem Beginn der Neuzeit in zunehmende Hörigkeit von der mathematischen Phy-

sik geraten war, die immer nachdrücklicher und uneingeschränkter zum Vorbild jeder wahren Wissenschaft erklärt wurde. In der Physik „geschieht" aber alles, ohne dass etwas „getan" würde, und selbst der Begriff des Bewirkens, der im Begriff der physischen Kausalität enthalten ist, bleibt in der Physik ein metaphysisches *skeleton in the cupboard*. Denn schon die Frage nach Wesen und Art der „Kräfte" bereitet dem Physiker ein gewisses Unbehagen, und er fühlt sich erst wieder wohl, wenn er den Begriff der Kraft ontologisch „einklammern" und rein phänomenologisch auf eine Massbestimmung der Beschleunigung zurückführen kann. Diese Scheu vor dem Begriff der Kraft hat die Psychologie gelehrig übernommen. Das zeigt sich nicht nur in der ostentativen Verachtung, die dem Begriff der Seelenvermögen lange Zeit hindurch entgegengebracht wurde, sondern vor allem in der Vernachlässigung einer Tatsache, auf die vielleicht erst wieder v. Uexküll mit dem gebührenden Nachdruck hingewiesen hat, dass nämlich die seelischen Eindrücke keineswegs wahllos und zwangläufig aus der Umwelt aufgenommen oder gemerkt werden, sondern lediglich soweit, wie sie für das Individuum Bedeutung besitzen, d.h. soweit, wie sie es zu einem bestimmten Verhalten oder Wirken anregen.

Wenn daher die Seele durchaus nicht der *camera obscura* gleicht, in der sich jeder beliebige Gegenstand abbildet, solange er nur überhaupt in den Bereich der Objektivöffnung gekommen ist, sondern viel eher einem Empfänger, der nur auf bestimmte Wellenlängen eingestellt ist, so muss doch von Anfang an im Auge behalten werden, dass diese Selektivität, welche das kennzeichnende Merkmal alles Lebens zu sein scheint, natürlich eine „transzendentale" Selektivität im Sinne Kants darstellt, sofern sie „Erfahrung überhaupt erst möglich macht": denn die Behauptung, dass der Organismus die Gegenstände der Umwelt kennte und sich erst auf Grund dieser Kenntnis entschiede, welche von ihnen er bemerken oder zur Kenntnis nehmen wolle, enthielte natürlich einen Widerspruch. Jene transzendentale Selektivität erscheint vielmehr nur verständlich unter Voraussetzung einer gegenseitigen Abstimmung oder, wenn man will, einer prästabilierten Harmonie zwischen Seele und Umwelt, zumal wenn man unter Seele im ursprünglichen Sinn des Wortes das Prinzip alles Lebens versteht. Ist aber diese Abstimmung dadurch gekennzeichnet, dass es von der Wirkwelt

des Individuums abhängt, welche Gegenstände der Umwelt in seine Merkwelt eingehen, d.h. als bedeutsam vermerkt werden, und besteht die Bedeutung eines Gegenstandes darin, dass er das Individuum auffordert, ihn in bestimmter Weise zu behandeln, so ist die elementare Funktion des Seelenlebens nicht das bloss passive Empfangen von Eindrücken, sondern das Vernehmen von Forderungen, die von den Gegenständen an die Seele gerichtet werden. Sobald sich daher die Psychologie entschliesst, von der irreführenden Metapher einer optischen Abbildung der Umgebung in der Seele zu der Metapher ihres akustischen Vernehmbarwerdens überzugehen, wird sie finden, dass ein solches Erlebnis des Vernehmens beides in sich vereinigt: das Erregtsein durch den Eindruck des Gegenstandes und zugleich das Gespanntsein auf die Bewältigung der von dem Gegenstand gestellten Aufgabe. Zur Kennzeichnung dieses Erlebnisses scheint jedoch kein anderer Ausdruck so geeignet wie das Wort Affekt, weil seine ursprüngliche Bedeutung den pathischen Charakter des Angetanseins ganz ebenso zum Ausdruck bringt wie seine später erworbene Bedeutung den drastischen Charakter des Tätigkeitsdranges.

Nicht der Eindruck im Sinn eines Bildes, das die Seele von einem Gegenstand empfängt, sondern der Affekt im Sinn der Erregung und Spannung, welche die Begegnung mit einem bedeutsamen Gegenstand in der Seele erzeugt, ist also das seelische Urerlebnis, welches als Tätigkeitsbereitschaft sogar dem Erlebnis der Tätigkeit selbst, jenem eigentümlichen *fiat*, vorausgeht, mit dessen Vollzug die Erregung in Beruhigung, die Spannung in Lösung umschlägt. Die Frage, worin das Erlebnis der Tätigkeit seinerseits besteht, lässt sich am leichtesten beantworten, wenn man von der körperlichen Tätigkeit und ihrem physikalischen Äquivalent, also einem Aufwand oder Umsatz von Energie ausgeht. Denn wenn Energie oder Arbeit von der Physik als das Produkt aus Kraft mal Weg definiert wird, so führt dieser Ansatz in doppelter Richtung weiter: Dass die Formel als Faktor den Weg enthalt, bedeutet, dass jeder physikalische Kraftaufwand einen Bewegungsvorgang erfordert; ganz ebenso setzt aber jede seelische Tätigkeit, die als solche erlebt wird und nicht zum mechanischen Reflex verkümmert ist, eine Gemütsbewegung als erregendes Moment voraus. Wie umgekehrt der Bewegungsvorgang nicht nur das Zeichen eines Kraftaufwan-

des, sondern auch die Grundlage seiner quantitativen Bestimmung bildet, so muss auch die Psychologie von dem körperlichen Bewegungsvorgang als dem Ausdruck einer gleichzeitigen Gemütsbewegung ausgehen, um zu einer Deutung der wirksamen seelischen Triebkräfte vorzudringen. Gemütsbewegungen und Triebkräfte sind aber nichts als andere Namen für Affekte. Dem wiederholt unternommenen, aber noch zu keinem befriedigenden Abschluss gebrachten Versuch einer Klassifikation der Affekte könnte daher vielleicht ein besserer Erfolg beschieden sein, wenn er einer Analyse der Ausdrucksbewegungen grössere Wichtigkeit beilegte, als dies bisher geschehen ist. Gegenwärtig verhält es sich so, dass entweder jede Hoffnung auf eine systematische Klassifikation der Affekte aufgegeben und an ihre Stelle eine „rhapsodische" Zusammenraffung auf Grund des äusseren Handlungserfolges gesetzt wird (so besonders in Amerika) oder dass die Klassifikation von einer phänomenologischen Analyse der Affekte selbst ausgeht und sich zu ihrer Ordnung ungeklärter psychologisch-philosophischer Kategorien bedient (so besonders in Deutschland). Wenn demgegenüber auf den Vorzug einer sozusagen behavioristischen Methode hingewiesen wird, welche mit einer Deutung der körperlichen Bewegungen als Ausdruckserscheinungen einsetzt, so kann dabei freilich weder von Behaviorismus im Sinn einer Beschränkung auf die blosse Beschreibung des körperlichen Verhaltens die Rede sein, noch von Ausdruckserscheinungen im Sinne der so vielfach komplizierten Körperbewegungen und -haltungen, welche den Gegenstand der eigentlichen Ausdruckswissenschaften Mimik, Physiognomik, Graphologie usw. bilden. Wie vielmehr eine analytische Bestimmung der geometrischen Figuren nicht mit den kompliziertesten Formen der Ornamentik statt mit den einfachsten geometrischen Gebilden (Kreis, Ellipse, Parabel, Hyperbel) beginnen wird, so abwegig erscheint es, bei einer Bestimmung der Affekte von unübersichtlich zusammengesetzten Affektkomplexen statt von Uraffekten auszugehen, die ihrerseits in Verbindung mit den mannigfaltigsten seelischen Erlebnissen treten und dadurch die mannigfaltigsten Modifikationen erleiden, dennoch aber bis in die höchsten Äusserungen des seelischen Lebens hinauf verfolgt werden können. Umgekehrt wird der Zugang zu diesen Uraffekten erleichtert, wenn es gelingt, gewisse Urbewegungen und -haltungen festzustellen, die

einen eindeutigen Ausdruckswert besitzen, und die Frage nach einer Ableitung der Uraffekte aus den Urbewegungen wird dann im wesentlichen mit der Frage nach einem Prinzip zusammenfallen, welches ihre „natürliche" Klassifikation gestattet.

Den ersten Schritt zu einer solchen Klassifikation hat Karl Bühler mit seiner Prägung des Begriffes von „Bezugswendungen" getan. Aber dieser Begriff ist deshalb zu eng, weil er nur die ursprüngliche Ausrichtung des Organismus gegen einen bedeutsamen Gegenstand anzeigt, während er die Fälle ausser Betracht lässt, in denen die eigentliche Tätigkeit erst dann beginnt, wenn die durch die Bezugswendung eingeleitete translatorische Bewegung ihr Ende erreicht hat und der Organismus mit seinem Bezugsgegenstand in Berührung gekommen ist.

Eine Klassifikation der Bewegungen, in denen sich die „Stellungnahme" des Organismus zu seinen Umweltsgegenständen ausdrückt, wird daher zweckmässig mit einer Unterscheidung zwischen idiokinetischen und heterokinetischen Bewegungen beginnen, ja nachdem, ob der Zweck der Tätigkeit bereits dadurch erreicht wird, dass sich der Organismus selber in Bewegung setzt, oder erst dadurch, dass er — ganz allgemein gesprochen — den Gegenstand in Bewegung setzt: denn jeder Energieaufwand, den der Organismus dadurch leistet, dass er mit dem Umweltsgegenstand „etwas anfängt", kann ja wiederum nur nach der Formel Kraft mal Weg erfolgen, selbst wenn es sich um die Vernichtung des Gegenstandes, also um die Zerstörung des Zusammenhanges seiner Teile handelt. Bei den idiokinetischen Bewegungen bildet also der Gegenstand, bei den heterokinetischen der Organismus den Richtpunkt, dem sich die Bewegung entweder annähert oder von dem sie sich entfernt. Das Schema der ursprünglichsten „Ausdrucksbewegungen" würde sich also folgendermassen darstellen:

	Organismus	Umwelts-gegenstand	Ergebnis
Heterokinese	1. ⊙	← ·	Ingestion
	2. ⊙	· →	Ejektion
Idiokinese	3. ← ·	⊙	Rezession
	4. · →	⊙	Profusion

Der Sinn dieser Bewegungen lässt sich in Worten also etwa folgendermassen bestimmen:

1. Her mit dir zu mir!
2. Fort mit dir von mir!
3. Fort mit mir von dir!
4. Hin mit mir zu dir!

Wenn wir zunächst von dem Bewegungstypus 4 absehen, der eine besondere Besprechung erfordert, ist est nicht allzu schwer, passende Namen für die Affekte zu finden, die sich in den drei übrigen Bewegungstypen ausdrücken, nämlich für

1. *Gier*

2. *Widerwillen* (wobei allerdings der Begriff in seinem ursprünglichen Sinn als Erlebnis eines drastischen gegen einen Umweltsgegenstand gerichteten Strebens und nicht eines bloss pathischen Abscheus verstanden werden muss)

3. *Furcht*

Man sieht also, dass nicht nur „Hunger und Liebe" das Getriebe erhalten, sondern dass die Aufzählung der Uraffekte unvollständig bleibt, solange man die Furcht und den Widerwillen nicht miteinrechnet. Man sieht aber zugleich, dass die Erregung, welche in diesen Affekten erlebt wird, entsprechend den beiden Grundrichtungen der Bewegungstypen wiederum eine doppelte ist, nämlich bei 1) eine Lockung, bei 2) und 3) eine Drohung, die von dem Gegenstand ausgeht, und dass die mit der Erregung gekoppelte Spannung so lange andauert, bis durch einen der oben angeführten Akte das angestrebte Verhältnis zum erregenden Umweltsgegenstand hergestellt ist. Die Polarität von Lockung und Drohung in den Uraffekten bildet daher nur den Ausdrucksgehalt der Polarität der Richtungen in den Urbewegungen, und diese Polarität enthüllt sich zugleich als Voraussetzung der Lebensfähigkeit, denn ein Organismus, für den sämmtliche Umweltsgegenstände ohne Ausnahme die Bedeutung „Heran!" besässen, wäre durch seine Gier ebenso dem Untergange geweiht wie ein Organismus, für den sämtliche Umweltsgegenstände mit der Urbedeutung „Fort!" versehen wären, durch seine Furcht.

Weiterhin scheint sich aus einer Analyse der Urbewegungen

zu ergeben, dass der Affekt des Widerwillens nicht so unmittelbar sein kann wie der Affekt der Gier und erst recht nicht der Affekt der Furcht. Denn wenn sich der Affekt der Furcht in einer idiokinetischen Bewegung äussert, so bedarf der Organismus zur Ausführung einer solchen Bewegung ja nur der Organe, die zu seiner Lokomotion erforderlich sind, und selbst festsitzende Organismen verfügen im allgemeinen wenigstens über einen Bewegungsapparat, der ihnen ein Ausweichen oder Zurückziehen gestattet. Zur Befriedigung der Gier ist es dagegen notwendig, den Umweltsgegenstand durch einen Akt der Ingestion in den eigenen Wirkungsbereich hereinzuziehen. Es bedarf also gewisser Greiforgane im weitesten Sinn, und zu diesen Greiforganen müssen noch weitere Abwehr- und Angriffsorgane hinzukommen, wenn der Organismus den Umweltsgegenstand durch einen Akt der Ejektion aus seiner Wirksphäre entfernt. Indessen verwischt sich dieser Unterschied sowohl bei den Organismen, die überhaupt keine spezifischen Bewegungsorgane ausgebildet haben, wie bei den Organismen, deren Lokomotionsorgane gleichzeitig die Funktion von Greif- und Abwehrorganen übernehmen: dient der Amöbe die Entsendung von Pseudopodien sowohl zur Fortbewegung wie zum Umfliessen eines Nahrungsgegenstandes, so erfolgt umgekehrt die Ausstossung der Nahrungsrückstände durch eine sozusagen passive Ejektion, d.h. durch ein Wegfliessen des Körperplasmas von den „Exkrementen"; bei den Ziliaten dagegen obliegt den Wimpern nicht nur die Fortbewegung des Organismus im umgebenden Medium, sondern auch die Ingestion durch Einstrudelung der Nahrungskörper und die Ejektion durch Wegstrudelung der Nahrungsrückstände und unerwünschter Annäherungen, und so sind es ganz allgemein die Lokomotionsorgane, die sich allmählich zu spezifischen Greif- und Abwehr- oder Angriffsorganen entwickeln. Die Entwicklung besonderer Ingestionsorgane ist freilich eine spätere, mit der Zunahme der Organisation verbundene Erwerbung, während die Ausbildung spezifischer Abwehr- und Angriffsorgane (wie Zähne, Klauen u.dgl.) zwar schon in den Nesselorganen der Einzelligen vorweggenommen ist, im eigentlichen Sinn aber doch erst auf Grund einer weitgehenden Differenzierung der Körpergewebe möglich wird.

Die phylogenetisch verhältnismässig späte Entwicklung der Waffenorgane weist jedoch auf eine eigentümliche Aufspaltung

der Uraffekte hin. Es besteht nämlich offenbar nicht nur dem Ergebnis, sondern auch dem Erlebnis nach ein grundsätzlicher Unterschied, ob sich der Widerwille darin äussert, dass der Organismus einen widerwärtigen Gegenstand aus seiner Innenwelt oder aus seiner Umwelt auswirft — wobei die Grenze zwischen Innen- und Umwelt durch die Körperoberfläche gegeben wäre —, ob er also den Gegenstand bloss sozusagen aus seinem Lebensbereich auswürgt oder aus seinem Wirkbereich vertreibt, im äussersten Fall sogar vernichtet, um seiner Wiederkehr in diesen Wirkbereich ein für allemal vorzubeugen. Schon jenes Vertreiben setzt Angriffswaffen voraus (die eigentümlicherweise bei den niederen Tieren meist chemische und erst bei den höheren Tieren mechanische Waffen sind), und gar die Vernichtung eines Gegners nicht um der Beute, sondern um des Schutzes willen lässt sich erst verhältnismässig spät in der Tierreihe beobachten. Daraus ergibt sich nicht nur, dass die Flucht ein ursprünglicheres Verhalten zu sein scheint als der Angriff, sondern vor allem, dass im Affekt des Widerwillens deutlich zwei verschiedene Nuancen zu unterscheiden sind, die Nuance des *Abscheus* (die dem Ekel verwandt, aber nicht mit ihm identisch ist) und die Nuance der *Wut*, der sich der Gegenstand in einen Gegner verwandelt und die nur dadurch befriedigt werden kan, dass der Gegner — Platonisch gesprochen — verschwindet oder vernichtet wird.

Dieselbe Differenzierung nach dem Umfang des Tätigkeitsbereiches findet sich aber auch bei den übrigen bisher behandelten Affekten, und auch sie steht in Abhängigkeit von der phylogenetischen Entwicklung. So ist die Gier ursprünglich reine *Genussgier*, die sich darauf beschränkt, den lockenden Gegenstand der Innenwelt des Körpers einzuverleiben. Mit dem Aufstieg in der Tierreihe tritt jedoch neben die blosse Genussgier eine *Habgier*, die auf eine unmittelbare Einverleibung des lockenden Gegenstandes verzichtet, ihre Befriedigung vielmehr bereits darin findet, ihn der Umwelt des eigenen Wirkbereichnes einzugliedern. Diese Habgier erstreckt sich natürlich zunächst auf künftig zu verzehrende Nahrungsmittel und auf sie gehen die verschiedenen Arten der Nahrungsaufspeicherung zurück. Sie dehnt sich in der Folgezeit aber auch auf andre Organismen aus, die in die Rolle des „Untergebenen" oder wenigstens des „Niederen" gedrängt und vom „Vorgesetzten" oder „Höheren"

durch seine überlegene Kraft zu Dienstleistungen gezwungen werden, und bildet so als *Machtgier* den einen — aber freilich auch nur den einen — Grundaffekt des sozialen Verhaltnis.

Analog verhält es sich mit dem Affekt der Furcht, nur dass es hier zunächst so aussieht, als ob ein gewisser Gegensatz zwischen dem äusseren Verhalten und der inneren Einstellung bestünde. Äusserlich scheint nämlich die *Fluchtbereitschaft* in die Umwelt auszugreifen, die *Abwehrbereitschaft* sich dagegen in der Innenwelt zu verschanzen: von innen gesehen besteht jedoch der Sinn der Flucht in einer Sicherung der Innenwelt unter beliebigen äusseren Umweltsbedingungen, es wird also nicht eigentlich von der Umwelt, sondern nur von dem Umraum Gebrauch gemacht, die Abwehr dagegen benützt näher oder ferner gelegene Umweltsgegenstände zur Sicherung der Innenwelt, und der Anschein, dass dabei kein Ausgriff auf die Umwelt erfolge, kommt nur dadurch zustande, dass der beschränkte Bereich eines Einsatzes der eigenen Abwehrkräfte eine räumliche Beschränkung der Verteidigungsanlagen, im einfachsten und ursprünglichsten Fall auf ein eigenes „Gehäuse", erforderlich macht. Die phylogenetische Reihenfolge von Flucht- und Abwehrfurcht ist aus zwei Gründen schwer festzustellen: einerseits findet sich bereits bei niederen Tieren (so z.B. schon bei den Foraminiferen) eine Gehäusebildung, andererseits pflegt bei der Begriffsbestimmung der Abwehr das Schlagwort vom Angriff als der besten Verteidigung in dem Sinne mitzuspielen, dass man sich eine Defensive nur mit offensiven Methoden oder zum mindesten nur mit offensiven Waffen vorzustellen geneigt ist. (Als die Japaner im Kriege gegen die Russen zu Beginn des Jahrhunderts die Taktik des Eingrabens weit über das bisher bekannte Mass erweiterten, galt sie in den Augen der europäischen Militärkreise zunächst als eine durchaus unfaire Art der Kriegsführung). Macht man sich aber klar, dass „Wehr" und „Waffen" nicht unbedingt zusammengehören, sondern dass die Abwehr, das *arcere,* nur ein Gehäuse, eine *arx,* voraussetzt, so wird man bei der verhältnismässigen Seltenheit von Gehäusebildungen sagen dürfen, dass im grossen und ganzen die Fluchtreaktion der Abwehrreaktion durch Wehrbauten (zu denen auch das Nest gehört) vorangeht, während die „natürlichen" Angriffswaffen unter diesem Gesichtspunkt ihrerseits als lokal eingeschränkte Gehäusebildungen erscheinen.

Immerhin lässt sich aus den bisherigen Ergebnissen schon ein Doppeltes ersehen. Auf der einen Seite finden sich die Uraffekte Gier, Widerwille und Furcht auf allen Entwicklungsstufen des seelischen Lebens wieder, aber dieselben Faktoren, welche die Entwicklung bestimmen, haben unverbeidliche Veränderungen im Erlebnisgehalt der Affekte zur Folge. Wenn sich daher etwa die Fluchtfurcht in der Pubertät zur *Schüchternheit* sublimiert (in deren Begriff das Gescheuchtwerden noch unverkennbar nachklingt), die Abwehrfurcht zum *Trotz*, so kann diese Sublimierung nur auf Grund jener Reflexion erfolgen, die erforderlich wird, sobald die naive *Setzung* der Dingwelt problematisch geworden ist und das Individuum erst dadurch wieder das gestörte Verhältnis zu seiner Umwelt herzustellen vermag, dass es sich von ihr *absetzt*. Zugleich kündigt sich in den Begriffen Schüchternheit und Trotz bereits der Übergang von der vorübergehenden Gemütsbewegung zur dauernden Gemütshaltung an, wie sie in den entsprechenden Haltungen der *Nachgiebigkeit* und der *Beständigkeit* zum Ausdruck gelangen. Auch diese gehen freilich noch auf die Wurzeln der Flucht- und der Abwehrfurcht zurück, aber sie erscheinen deshalb weder als Minderwertigkeiten noch als polare Gegensätze, weil sie erst dann auftreten können, wenn das Individuum sich nicht mehr bloss im Reifungsprozess von der Dingwelt *absetzt*, sondern die voll ausgereifte Persönlichkeit sich für die Verwirklichung absoluter Werte in der Dingwelt *einsetzt*. Nicht eine zur Gewohnheit erstarrte, sondern eine jedesmal neu vom Gewissen geforderte Haltung entscheidet darüber, wie weit das Individuum fremden Ansprüchen nachgeben oder wie weit es auf seinen eigenen Ansprüchen bestehen soll, und es ist ganz im Platonischen Sinne nur das Wissen um die absoluten Werte, welche das Wesen des Gewissens ausmacht.

Wie bei der Furcht, so lässt sich auch bei den übrigen Uraffekten ihre allmähliche Sublimierung zu Characterzügen verfolgen, doch würde es im vorliegenden Zusammenhange zu weit führen, diesen Nachweis im einzelnen zu erbringen.

Nicht unerwähnt bleiben darf dagegen die andere für die Affektpsychologie bedeutsame Tatsache, die sich bereits bei der Betrachtung der Uraffekte aufdrängte, dass sie nämlich verhältnismässig selten unvermischt, sondern meist in *Affektkomplexen* auftreten. Auf das nächstliegende Beispiel, die ursprüng-

liche Koppelung der Defensive mit der Offensive, also der Abwehrfurcht mit der Wut ist schon im Früheren aufmerksam gemacht worden: die Gefährlichkeit der *cornered rat* und des *man with his back to the wall* is sprichwörtlich, weil die Abwehr bei Verlegung des Fluchtweges nur durch die Vernichtung des Verfolgers glücken kann. Analog kombinieren sich etwa Habgier und Furcht (Urform des Geizes), Habgier und Wut (Urform der Raubsucht), und es würde wiederum zu weit führen, sämtliche Kombinationsmöglichkeiten der Uraffekte aufzuzählen.

Innerhalb der bisher behandelten Affekte besteht aber noch ein weiterer grundsätzlicher Unterschied, den man in möglichst allgemeiner Fassung als den Unterschied zwischen Konservativ- und Destruktiv- oder zwischen Wahrungs- und Zerstörungsaffekten bezeichnen könnte. Zu den Zerstörungsaffekten gehören Gier und Widerwille in gleichem Masse, denn beide zielen auf die Zerstörung ihres Gegenstandes ab. Das gilt nicht nur für die Genussgier und die Wut, weil das Verzehren eines Gegenstandes seine Zerstörung ganz ebenso bewirkt wie seine schiere Vernichtung, sondern auch die Habgier zerstört die ungehemmte Wirkfähigkeit des Überwältigten und der Abscheu stösst nur aus, was durch Zerstörung seiner Wesenseigenschaften bereits abstossend geworden war. Demgegenüber geht die Furcht unter allen Umständen auf Wahrung des Lebens- oder Wirkbereiches aus, sei es dass sie durch Flucht oder durch Abwehr erfolgt. Ist aber die ursprünglichste Form der Abwehr der Bau eines „Gehäuses", wird also die Konservation zu einer Konstruktion, so eröffnet sich damit der Ausblick auf eine höhere Stufe der seelisch-geistigen Tätigkeiten, auf der das bloss destruktive „Tun" der Genussgier und der Wut durch ein konstruktives „Machen" als die Grundlage aller *künstlerischen Gestaltung* abgelöst wird.

Dabei bestätigt sich freilich wiederum die Tatsache, dass die Affekte kaum jemals vereinzelt, sondern fast immer nur in Komplexen auftreten. Denn wenn auch die konstruktive Tätigkeit mit dem Gehäusebau anzuheben scheint, so erfordert doch schon die Anfertigung künstlicher Gehäuse eine Zerstörung natürlich gewachsener Umweltsgegenstände, so dass selbst die konservativen eine geradezu unlösliche Verbindung mit den destruktiven Affekten eingehen. Das zeigt sich auch in den

Komplexbildungen zwischen den konservativen und den gewissermassen sublimiert-destruktiven Affekten. Denn mit dem Anstieg in der Tierreihe erweitert sich der blosse Gehäusebau für das einzelne Individuum zum Nestbau für die Familie, in dem die Schutzsuche bereits eine Verbindung mit der sublimierten Habgier eingeht: der Habgier der Männchens, sich eines Weibchens, und des Weibchens, sich der Jungen zu „bemächtigen". Gerade bei diesen Brutpflege-Instinkten wird es aber besonders deutlich, dass eine erschöpfende Analyse des Verhaltens mit den bisher behandelten *Selbstbehauptungsaffekten* nicht ihr Auslangen finden kann, sondern durch den Aufweis eines Affektes der *Selbsthingabe* ergänzt werden muss.

Damit ist die Untersuchung von selbst zu dem 4. Uraffekt zurückgekehrt, dem bisher noch nicht einmal ein Name gegeben wurde. Diese Schwierigkeit wird sich auch in der Folge nicht verringern, und so könnte es zunächst scheinen, als ob der 4. Uraffekt nur aus Gründen der Symmetrie zur Abrundung der Architektonik in das System der Uraffekte künstlich eingeschaltet worden wäre. Um diesen Verdacht zu zerstreuen, ist es vielleicht am zweckmässigsten, zu seiner Bestimmung den umgekehrten Weg einzuschlagen, also von der höchsten Sublimierung des Uraffektes auszugehen, der sich in der Bewegung „Hin mit mir zu dir!" ausdrückt. Diese Bewegung ist in ihrer reinen Ausprägung so unmissverständlich eine Bewegung der Hingabe, dass der Affekt, der ihr zugrunde liegt, nur mit dem Wort *Liebe* angemessen bezeichnet werden kann. Denn wie immer man Liebe beschreiben will, man endet doch immer bei dem Bild eines Stromes, der die Dämme des eigenen Ich durchbricht, sei es dass er aus dem eigenen Ich hervorquillt und sich in den Schoss eines anderen Ich ergiesst, sei es dass er aus der Tiefe eines anderen Ich aufsteigt und die Bollwerke überflutet, mit denen sich das eigene Ich gegen die Umwelt abgesetzt und gesichert hatte. Es ist natürlich kein Zufall, dass dieser Fluss und Widerfluss von Ich zu Ich im Geschlechtsakt gleichsam symbolisiert erscheint. Aber die blosse Vorläufigkeit der Geschlechtsliebe kennzeichnet sich gerade dadurch, dass sie nicht ohne die Gier des Besitzenwollens zu denken ist und ihre Befriedigung eben deshalb immer in die berüchtigte *tristitia*

ausklingt. Aber nicht in dem besitzergreifenden Umfangen liegt das Wesen der Liebe, sondern im seligen Verströmen des eigenen und dem Einströmenlassen des anderen Ich, und so wenig jenes Verströmen dem Auswürgen des Abscheus, so wenig gleicht dieses Einströmenlassen dem Einsaugen der Gier: das Gefäss nimmt seinen Inhalt auf, ohne ihn einzuschlucken, und von hier aus fällt sogar Licht auf eine sonderbare Redewendung im 1. Thessalonicherbrief (4, 4), wo Paulus die Frau als das „Gefäss" (vas, σκεῦος) des Mannes bezeichnet, denn dieses Bild wirkt schlechterdings anstössig, solange sich der Blick nicht über die Sexualsphäre zu erheben vermag.

Beruht die tragische Dämonie der Geschlechtsliebe gerade auf der unlösbaren Verknüpfung von Liebe und Gier, so geht doch die wesenhafte Verschiedenheit beider Affekte aus dem für die Reifungszeit kennzeichnenden Liebeserlebnis des *Schwarmes* hervor, das sich gerade durch den Mangel jeder besitzlüsternen Gier auszeichnet und eine der reinsten Ausprägungen völlig selbstloser Liebe darstellt, die dem in seiner Selbstsucht erstarrten Erwachsenen wenigstens in der Erinnerung noch zugänglich zu sein pflegt.

Dagegen scheint sich das Phänomen der Selbsthingabe in die tieferen Entwicklungsstufen der menschlichen Kindheit oder gar des tierischen Verhaltens zunächst überhaupt nicht weiter verfolgen zu lassen. Das trifft insofern bis zu einem gewissen Grade zu, als die volle Ausbildung des Affektes der Selbsthingabe natürlich die Ausbildung eines Selbstbewusstseins voraussetzt, die erst beim Menschen durch die Reflexion der Merktätigkeit von der Umwelt auf das eigene Ich während der Pubertät zustande kommt. Immerhin *lebt* der Mensch ganz ebenso wie das Tier sein eigenes Selbst, bevor er es *erlebt*, und dieses unmittelbare Gegenwärtigsein des Selbst in jeder Tätigkeit ist es auch, was vor jeder Reflexion den Boden für den Affekt der Selbsthingabe bilden kann. Freilich ist es selbst beim Kinde schwer, Ausdrucksbewegungen festzustellen, die als eindeutiger Ausdruck eines Affektes der Liebe gelten dürften: immerhin ist die Geberde des satten oder müden Kindes, das sich in den Arm oder in den Schoss der Mutter schmiegt, gewiss nicht nur als Schutzsuche zu deuten, also auf den Affekt der Abwehrfurcht zurückzuführen, sondern in ihr drückt sich bei unvoreingenommener Betrachtung deutlich genug der Affekt der Liebe im

Sinn der Selbsthingabe aus; ganz ebenso bedeutet die Geberde der Mutter, die das Kind in den Arm oder auf den Schoss nimmt, durchaus nicht nur den Akt der Besitzergreifung, zeigt also nicht bloss den Affekt einer sublimierten Habgier an, sondern ist Ausdruck eben jenes liebevollen In-sich-Einströmenlassens, in dem das Verströmen erst seine Erfüllung findet.

Nun lassen sich derartige „Brutpflege-Instinkte" natürlich weit hinab in der Tierreihe verfolgen und es wird daher im Lichte der vorhergehenden Überlegungen nicht mehr ganz so abwegig erscheinen, in ihnen Ansätze zu einem Affekt zu suchen, der auf höheren Stufen den Namen der Liebe verdient. Wie aber dieser Affekt auf den niederen Stufen angemessen zu bezeichnen wäre, ergibt sich vielleicht aus der primitivsten Form jenes Flusses und Widerflusses, die sich in der belebten Natur beobachten lässt, nämlich in der Konjugation, dem eigentümlichen und von der Kopulation aller übrigen Tiere verschiedenen Befruchtungsvorgang der Ziliaten. Denn wenn das Wesen der geschlechtlichen Fortpflanzung darin besteht, eine Ingestion der ejizierten männlichen in die weiblichen Geschlechtszellen (wo die Voraussetzungen dazu gegeben sind mittels Ingestion der männlichen in die weiblichen Geschlechtsorgane) herbeizuführen, findet bei der Konjugation ein Austausch der Geschlechtszellen zwischen beiden Geschlechtspartnern statt, indem nach der Reduktionsteilung des Grosskernes jeder Geschlechtspartner seine eigene Kernhälfte in den anderen Geschlechtspartner verströmt und zugleich dessen Kernhälfte in sich einströmen lässt: ich wüsste nicht, durch welches Bild die beiden Seiten des Liebesaffektes in ihrem Fluss und Widerfluss besser dargestellt werden könnten. Dann ist es aber bei sinngemässer Deutung durchaus keine Blasphemie, denselben Uraffekt im einzelligen Klümpchen lebendiger Substanz wirksam zu denken wie nach den Worten Meister Eckhardts bei der „Wiedereinbildung in das einfältige Gut", solange man sich der wesenhaften und nicht bloss graduellen Verschiedenheit der seelisch-geistigen Entwicklungsstufen bewusst bleibt. Freilich bleibt es dann noch immer eine offene Frage, wie dieser Uraffekt auf der untersten Stufe des Seelenlebens benannt werden soll: ich würde vorschlagen, ihn schlechthin den Affekt der *Erschliessung* zu nennen, solange sich keine angemessenere Bezeichnung findet, weil sich ein Inneres sowohl dann erschlies-

sen muss, wenn es seinen eigenen Inhalt verströmt, wie wenn es einen fremden Inhalt in sich einströmen lässt.

Es ist auch noch ein anderes, was den Ausdruck Erschliessung als Voraussetzung und Ergebnis der Ergiessung empfiehlt, dass nämlich alles Strömen nur aus der Tiefe kommt und zur Tiefe fliesst, dass also das *profundum* nur aus der *profusio* quillt und nur der *profusio* erreichbar ist. Das ist deshalb mehr als ein blosses Wortspiel, weil der Affekt der Erschliessung als Affekt der Selbsthingabe allen anderen Uraffekten als Affekten der Selbstbehauptung gegenübersteht und gerade deshalb den Ansatzpunkt zu einer Höherentwicklung als einer gleichzeitigen Vertiefung des seelisch-geistigen Lebens darstellt: ist es erst die Hingabe an das Du, in der dem Reifenden das Erlebnis des eigenen Ich zum erstenmal aufleuchtet, so ist es wiederum nichts anderes als die höchste Form der Erschliessung, wenn sich die Persönlichkeit den sachlichen Forderungen der absoluten Werte öffnet. Damit kennzeichnet sich der Affekt der Erschliessung zugleich als Voraussetzung für die Emanzipation des Merkens vom Wirken, also für eine Verselbständigung des *Erkennens*, das sich nun nicht mehr darauf beschränkt, das Streben durch den Nachweis bedeutungsvoller Gegenstände zu „erregen", sondern über die Urbedeutungen hinaus nach dem „Wesen" der Dinge fragt. Mag diese Frage noch so unbestimmt gestellt sein, mag sich insbesondere der Begriff des Wesens noch nicht vom Begriff des Scheines, geschweige denn vom Begriff der Erscheinung mit hinreichender Deutlichkeit abheben, so kann sie doch überhaupt erst dann gestellt werden, wenn jenes Das, nach dessen Was gefragt wird, als ein bereits Dagewesenes wiedererkannt wird (Aha-Erlebnis). Dass bedeutet aber nicht nur eine Einbeziehung der Zeit in die Merkwelt, denn im Erlebnis des Wieder liegt zunächst das Erlebnis der Vergangenheit eingeschlossen, sondern auch eine Identifizierung jenes Das als eines *Selben* und damit die Entdeckung der ersten unsinnlichen Kategorie, die zur Formung der sinnlichen Materie Verwendung findet. (*αὐτό* = *αῦ τό* = wieder das; „phonognomisch" bringt bereits der Ausruf „Aha" denselben Tatbestand zum Ausdruck, indem er das „Ah" des Stutzens mit dem „Ha" des Bewältigens verquickt). Logisch ist natürlich die Erkenntnis des „Selben" nur zusammen mit seiner Unterscheidbarkeit vom „Anderen" gegeben, psychologisch scheint jedoch auch hier der Primat der

Negation, d.h. die Gesetzmässigkeit zu gelten, dass dem Erlebnis des Selben das Erlebnis des Anderen vorhergehen muss. Dafür spricht die Tatsache, dass es im Grunde nur eine eindeutige Ausdrucksbewegung des Merkvorganges gibt, nämlich das *Stutzen* des Aufmerksamwerdens, das seiner motorischen Seite nichts anderes ist als das Annehmen einer Bewegungsbereitschaft unter Hemmung der instinktiven Bewegungsimpulse. In dieser Hemmung liegt ein Dreifaches eingeschlossen: erstens, dass in der Haltung des „noch nicht!" auch die Dimension der Zukunft in das Erlebnis eingeht; zweitens, dass dem Erlebnis des Stutzens ein sehr starker affektiver Gehalt zukommt, nämlich das Erlebnis einer unlustvollen oder zum mindesten unbehaglichen Unheimlichkeit des Anderen, die im Ausdruck Stutzen (von der Wurzel Stossen) wie in den Ausdrücken *stupere* (von der Wurzel *τυπ-*, also gleichbedeutend mit Geschlagensein, *ἐκπλήττεσθαι*), *start* oder *startle* (wurzelverwandt sowohl mit [aufwärts] Starren wie mit [abwärts] Stürzen, vgl. das Heideggersche Geworfensein) und besonders anschaulich in *tressauter* zum Ausdruck gelangt und in dem Erlebnis der lustvollen oder wenigstens behaglichen Vertrautheit des Selben sein Gegenstück findet; und schliesslich, dass jener durch die Hemmung der instinktiven Affekte erzeugte Bedeutungswandel der Umweltgegenstände die Voraussetzung für die Ausbildung der Gefühle darstellt, in denen erst die polare Spannung zwischen dem Selbst als dem zuletzt entdeckten, aber dann am unerschütterlichst gewahrten Selben und allem „Anderen" ihrerseits erlebt wird. Im Erleben der „Heimlichkeit" des Selben — über den Doppelsinn des Wortes Heimlichkeit liesse sich eine eigene Abhandlung schreiben — und der Unheimlichkeit des Anderen offenbart sich also ein Affekt, der nicht nur auf einer höheren Stufe als der des Instinktes steht, weil er sich erst auf der Grundlage des Zeiterlebnisses entwicklen kann und seinerseits die Gefühle aus sich hervorgehen lässt, sondern auch zugleich als einziger Affekt die angeblich interesse- oder willensfreie „Kontemplation" aller sachlichen Wertordnungen bestimmt und beseelt. Behält man aber im Auge, dass schon das Stutzen vor dem Anderen als Vorstufe zur Erkenntnis des Selben jene Emanzipation von der Zwangsläufigkeit der Selbstbehauptungsinstinkte erfordert, welche die Voraussetzung für die Wirksamkeit des Erschliessungsaffektes bildet, so kann es am Ende nicht

mehr wundernehmen, wenn im Innewerden der Grundlage aller Wertordnungen die *visio* mit dem *amor Dei* zusammenfliesst.

Dagegen wird man vielleicht fragen, ob denn nicht *Lust* und *Unlust*, von denen erst im letzten Absatz andeutungsweise die Rede war, in noch viel höherem Mass als die genannten den Namen von Uraffekten verdienen. Um indessen das Wesen von Lust und Unlust richtig zu verstehen, muss man die Uraffekte selbst noch einer genaueren Betrachtung unterziehen. Sind nämlich die Affekte Tätigkeitserlebnisse, so bedeutet die Überwindung eines Widerstandes, welche das Wesen der Tätigkeit ausmacht, nicht nur die Überwindung einer Gegenkraft, sondern auch die Überwindung einer Raumstrecke. Die gegebene Ableitung der Uraffekte gilt daher für alle freibeweglichen Organismen. Bei festsitzenden, also entweder überhaupt nicht oder nur beschränkt beweglichen Organismen dagegen schalten Tätigkeiten aus, die eine Ortsveränderung des Gesamtorganismus erfordern würden, und wenn die Natur es zuliesse, dass die Gegenstände der Umwelt für solche Organismen eine lockende oder drohende Bedeutung besässen, wäre sie ebenso grausam wie ein Folterknecht, der seinem angeketteten Opfer die Nahrung ausserhalb der Reichweite stellte oder aus der Ferne mit Waffen auf ihn zielte. Für die festsitzenden Organismen können vielmehr Umweltsgegenstände nur bei unmittelbarer Berührung Bedeutung erlangen, und in diese *Nahaffekte* (wie man sie nennen könnte) münden schliesslich auch bei freibeweglichen Organismen die *Fernaffekte* des Lockens und Drohens ein, sobald der begehrte, widerwärtige oder gefährliche Gegenstand mit dem Organismus in Berührung gekommen ist. Damit erweist sich schon in der Lehre von den Affekten eine Unterscheidung als bedeutsam, die von der Psychologie etwas über die Achsel angesehen zu werden pflegt, nämlich die Unterscheidung zwischen Nah- und Fernsinnen, aus der zugleich die enge Verbindung der Affekte mit ihren sinnlichen Grundlagen hervorgeht. Während sich also die Gegenstände, die sich dem Auge, dem Ohr und der Nase darbieten, niemals mit diesen Organen in unmittelbare Berührung kommen dürfen, können umgekehrt Gegenstände durch Haut und Zunge im allgemeinen nur bei unmittelbarer Berührung mit ihnen wahrgenommen werden: dann können aber von diesen Gegenständen natürlich keine Lockungen oder Drohungen mehr ausgehen, sondern die

Tätigkeiten des Einschluckens, Auswerfens oder Ausweichens, die alle nur ein Minimum an Bewegung erfordern, müssen unmittelbar durch die Eigenschaften des berührenden Gegenstandes ausgelöst werden. An die Stelle der Lockung tritt also die Lust, an die Stelle der Drohung die Unlust, während es sich bei genauerer Beobachtung herausstellt, dass Erregung und Spannung im Höhepunkte von Lust und Unlust eigentlich überhaupt nicht mehr erlebt werden. Gewiss gibt es auch lust- und unlustvolle Erregungen und Spannungen, und es wird in der Folge noch weiter von ihnen zu reden sein. Aber die Nahlust und -unlust, die den Eindrücken des Haut- und Geschmacksinnes anhaftet, führt so unmittelbar in die Tätigkeiten der äusseren und inneren Kontaktsuche und der äusseren oder inneren Kontaktflucht über, dass bei erfolgreichem Vollzug der Tätigkeit Erregung und Spannung überhaupt nicht zur Entwicklung gelangen.

Dabei ist zu beachten, dass zwischen dem Tastsinn auf der einen und dem Temperatur- und Geschmacksinn auf der anderen Seite insofern ein Unterschied besteht, als die Befriedigung bei den beiden zuletzt genannten Sinnen schon durch den blossen Kontakt erzielt werden kann (dass die wohlschmeckenden Dinge gleich verschluckt werden, bedeutet innerhalb der Umgebung, welcher der Organismus angepasst ist, eine biologische Notwendigkeit, aber der Wohlgeschmack bleibt, auch wenn das Verschlucken künstlich verhindert würde: ja das Kosten beruht ja gerade auf einer derartigen künstlichen Hemmung des Schluckens), während eine volle Befriedigung der Kontaktsuche durch den Tastsinn deshalb nicht möglich ist, weil der Tätigkeit des Anschmiegens, wie wir gesehen haben, bereits die Einstellung der Profusion zugrundeliegt, die über den äusseren zu dem inneren Kontakt des Einströmens hindrängt. Das zeigt sich sowohl darin, dass schon das Anschmiegen nicht in Form einer kontinuierlichen Verstärkung des Druckes, sondern einer immer weiter getriebenen ruckweisen Annäherung vor sich zu gehen pflegt, wie auch vor allem darin, dass die Sexuallust als das intensivste Lusterlebnis, das durch den Tastsinn vermittelt wird, bei den höheren Wirbeltieren nur durch eine rhythmische Friktion der Genitalien, also durch einen immer wieder erneuten Versuch eines immer tieferen Eindringens, erzeugt wird. Dieser Sexuallust gegenüber erscheinen die übrigen lustvollen Erleb-

nisse des Haut- und Geschmacksinnes farblos, ja beinahe nur als Äusserungen eines stimmungmässigen Behagens — wovon sofort weiter unten —, und deshalb hat es seinen guten Sinn, wenn der deutsche Ausdruck Wohl-lust andeutet, dass die Sexuallust die höchste Ausprägung der sinnlichen Lust darstellt.

Nicht ganz übereinstimmend verhält es sich mit den Nahaffekten der Unlust hinsichtlich ihrer biologischen Bedeutung, denn bei der Regulierung der Ingestion und Ejektion durch den Geschmacksinn kommt es mehr darauf an, den hungrigen Organismus vor dem Verschlucken schädlicher Stoffe zu bewahren als ihn zum Verschlucken nützlicher Stoffe anzuregen („In der Not frisst der Teufel Fliegen!"). Aus unserem eigenen Erleben kennen wir aber einen Geschmack, der nicht nur das Schlucken verhindert, sondern geradezu das Auswürgen auslöst, nämlich den Geschmack des Bitteren in höherer Konzentration, und daher rührt es wohl auch, dass in allen Sprachen das Wort „bitter" die Bedeutung des schlechthin Unlustvollen angenommen hat. Immerhin steht sogar die Unlust des Bitteren an affektiver Intensität im allgemeinen hinter der Unlust des körperlichen Schmerzes zurück, dessen biologische Bedeutung sich darin zu äussern scheint, dass er bei geringeren Graden ein Zurückziehen der getroffenen Körperstelle, bei stärkeren Graden eine Abstossung der Schmerzquelle auslöst. Somit scheinen innerhalb der Nahaffekte Wollust und Schmerz die beiden Pole intensivster sinnlicher Lust und Unlust zu bilden.

Soviel ergibt sich also immerhin schon aus dem bisher Gesagten, dass Lust und Unlust in systematischer Hinsicht nicht (nach dem Vorgange Wundts) auf dieselbe Stufe des affektiven Lebens gestellt werden können wie Erregung und Beruhigung, Spannung und Lösung. Bei den Nahaffekten sind Lust und Unlust Erlebnisse, die gewissermassen der Erregung und Spannung vorausgehen, bei den Fernaffekten Erlebnisse, die der Beruhigung und Entspannung nachfolgen, ja die durch Drohung und Lockung hervorgerufene Spannung scheint geradezu durch eine Vorwegnahme des lust- oder unlustvollen Ergebnisses der Gegenstandseinwirking bestimmt zu sein. Eine solche Bestimmung ist freilich bei den Uraffekten unmöglich, weil sie ja bereits eine Leistung des Gedächtnisses voraussetzen würde, aber es zeigt sich immerhin schon an dieser Stelle, dass Lust und Unlust sozusagen den Brückenkopf bilden, von dem aus

der Vorstoss aus der Gegenwart in die Vergangenheit und Zukunft unternommen wird. Ja noch mehr: Lust und Unlust tragen nicht nur zur Eroberung der Zeit bei, sondern sie nehmen eine noch höhere geistige Tätigkeit voraus als Erinnerung und Phantasie, nämlich die Wahlenscheidung zwischen zwei Möglichkeiten des Handelns, sofern sich in dem Gegensatz zwischen Lust und Unlust der Gegensatz zwischen Ja und Nein andeutet. Umgekehrt gibt es Lust- und Unlusterlebnisse, für die sich die Bezeichnung Stimmungslust und -unlust deshalb empfiehlt, weil sie bis in die Vorphasen des Tätigkeitserlebnisses hinabreichen: die Unlust des erwachenden Bedürfnisses (Lockes *uneasiness*) und die Lust des Geborgenseins (die im Ausdruck Behagen anklingt) bilden gewissermassen den Quellgrund der ausgreifenden wie der sichernden Tätigkeit.

Dass mit der endgültigen Überwindung des durch den Gegenstand gesetzten Widerstandes nicht nur Erregung und Spannung in Beruhigung und Lösung umschlagen, sondern dass dieser Umschlag zugleich mit einer der Stärke des Widerstandes proportionalen Lust verbunden ist, bedarf ebensowenig weiterer Ausführungen, wie dass eine Verzögerung der Befriedigung, also das Andauern eines Widerstandes, dessen Überwindung die Kräfte des Individuums zu übersteigen droht, eine wiederum der Stärke des Widerstandes proportionale Unlust erzeugt. Daraus erklärt es sich auch, dass der Uraffekt der Erschliessung die engste Verbindung mit dem Erlebnis der Lust eingeht, weil es hier nicht der Anspannung eigener Kräfte zur Überwindung eines fremden Widerstandes sondern nur einer Entspannung der zur Sicherung des eigenen Lebensbereiches aufgewendeten Kräfte bedarf. (Die Schwierigkeiten, die einer solchen Entspannung bei introverter Verkrampfung entgegenstehen können, liegen natürlich oberhalb des seelischen Niveaus der Uraffekte.)

Die Frage, ob Erregung und Spannung selber mit einem ihnen eigentümlichen Lust- oder Unlustcharakter behaftet sind, ist deshalb so schwer zu beantworten, weil wir reine Drohungen und Lockungen, wie sie von den Urbedeutungen der Gegenstände ausgehen, kaum nachzuerleben vermögen, sondern ihrer statt dessen als Bedrohungen und Verlockungen innezuwerden, d.h. in Erinnerung und Phantasie das Ergebnis der durch Drohung und Lockung ausgelösten Tätigkeit vorwegzunehmen pflegen. Damit erhält die Bedrohung im allgemeinen einen un-

lustvollen, die Verlockung einen lustvollen Charakter. (Die Lust an der Bedrohung, die aus Langeweile oder aus dem Bedürfnis nach Überkompensation von Minderwertigkeitskomplexen entspringt, und die Unlust an lockenden Gegenständen aus Übersättigung oder Genussunfähigkeit entwickelt sich natürlich wieder erst in einer späteren Phase des seelischen Leben.) Sucht man sich aber in das Erlebnis der Urbedeutungen „Fort!" und „Heran!" zurückzuversetzen, wie es sich etwa den instinktiven Sicherungs- oder Ausgriffsakten zugrundeliegt (um diese Ausdrücke von Clauss beizubehalten), so wird man finden, dass es überhaupt keine merkliche Beimischung von Lust oder Unlust enthält, trotzdem es in der reflektierenden Erinnerung mit solchen Qualitäten ausgestattet erscheinen mag. Ebensowenig macht sich auf dem Niveau der Uraffekte ein Unterschied geltend, der für die Analyse der Lust-Unlust-Erlebnisse in späteren Phasen der seelischen Entwicklung Bedeutung erlangt, ob nämlich die Lust vorwiegend durch das Auskosten des durch die Überwindung eines gegenständlichen Widerstandes erreichten Ergebnisses oder bereits durch die Überwindung des Widerstandes selber erzeugt wird. Denn diese *Wirklust* oder Funktionslust ist natürlich Lust an der eigenen Machtbetätigung und setzt daher eine noch so keimhafte Selbstbesinnung voraus, in der sich das Merken vom Wirken bis zu einem gewissen Grad emanzipiert und sich vom Anderen auf das Selbst zurückgewendet hat. Indessen kann es freilich vorkommen, dass schon die Phase der Erregung und Spannung von kleinen Teilbefriedigungen durchsetzt wird, wie sie etwa bereits mit der Überwindung des „Reibungswiderstandes" bei der blossen Fortbewegung verbunden sind: aber wenn dadurch die ganze Erregungs- und Spannungsphase schon einen lustvollen Charakter zu erhalten scheint, so ist diese Lust doch keine reine Wirklust, sondern im wesentlichen wieder nur eine Ergebnislust.

Was sich dagegen auf der Stufe der Uraffekte bereits nachweisen lässt, ist die zuvor so genannte Stimmungslust und -unlust, nur dass diese wiederum keine Affekte sind, sondern gewissermassen der Mutterboden, aus dem erst die Affekte hervorwachsen. Der Name Stimmungen ist für diese Erlebnisse deshalb so angemessen, weil sie tatsächlich auf einer Abstimmung, einer Resonanz des Individuums auf seine Umgebung beruhen. Das Vorhandensein einer solchen Resonanz lässt sich

am leichtesten in dem Erlebnis des *Unbehagens* aufweisen, das bei ihrer Störung auftritt. Wenn die Störung durch die Einwirkung eines Umweltgegenstandes erfolgt und als solche zum Bewustsein gelangt, wird die Entwicklung des Unbehagens allerdings durch die Erregung und Spannung verhindert, welche von der Drohung oder Lockung ausgeht. Wenn aber schon die Physiologie nicht ohne Berücksichtigung der „inneren Stimmung" des Organismus bei der Voraussage des Reizerfolges auskommen kann, so darf die Psychologie erst recht nicht übersehen, dass die körperlichen Vorgänge des Stoff- und Energiewechsels als Ausdruckserscheinungen seelischer Vorgänge zu betrachten sind und dass der Rhythmus, welcher jene körperlichen Vorgänge beherrscht, auch den Rhythmus des seelischen Lebens bestimmen muss. Der Urrhythmus der körperlichen Vorgänge schwankt zwischen den Polen der Ermüdung und der Erholung einerseits, der Bedürftigkeit und der Bedürfnislosigkeit andererseits. Wie aber die Zustände der Erholung und der Bedürfnislosigkeit an sich lustvollen, die der Ermüdung und der Bedürftigkeit an sich unlustvollen Charakter tragen, so ist doch das Unbehagen der Ermüdung nicht mit der Unlust der vergeblichen Bemühung, das Unbehagen des noch ungerichteten Bedürfnisses nicht mit der Unlust der frustranen Erregung und Spannung zu verwechseln. Ebensowenig fällt das lustvolle Behagen an der Erholung mit dem allmählichen Wiedererwachen des Interesses an lockenden Gegenständen, noch viel weniger die Lust der Bedürfnislosigkeit des an die Welt verlorenen und zugleich in ihr geborgenen Wesens mit der Lust gesättigter Gier zusammen; denn obgleich die Lust der Bedürfnislosigkeit die Lust der Sättigung zu ihrer Voraussetzung hat, so vermag sie doch das Erlebnis der Sättigung nicht nur wesentlich zu überdauern (ohne in Langeweile umzuschlagen, was Schopenhauer in seinem Pessimismus übersehen hat), sondern besitzt auch in ihrer „Weltabwendung" eine so ausgeprägte Eigentümlichkeit, dass sie nicht mit der blossen Sättigungslust verwechselt werden sollte. Eben jene Urstimmung des Behagens, die nur von aussen durch Drohungen oder Lockungen, von innen durch Ermüdung und Bedürfnis gestört werden kann, nimmt aber in ihrer Verquickung des Erlebnisses von Verlorenheit und Geborgenheit die Polarität des Verströmens und Einströmens

auf einer Stufe voraus, die dem „Reich der Mütter" um so vieles näher liegt wie die Stimmung dem Affekt.

Das Verhältnis zwischen den Lust-Unlust-Erlebnissen und den Affekten ist also in etwa dem Verhältnis zwischen spezifischen Organfunktionen und den Hormonen der zugehörigen Organe zu vergleichen, die einerseits von dem voll funktionierenden Organ produziert werden, deren Produktion aber andererseits die Voraussetzung für die Vollfunktion des Organs bildet. Wenn deshalb Lust und Unlust ebensowohl als Folge- oder Begleiterscheinung wie als Stimmungsgrundlage der Affekte auftreten, so scheint es vielleicht nicht allzu weit hergeholt, sie als „Affekthormone" von den eigentlichen Affekten abzuheben.

Die vorstehenden Ausführungen haben ihren Zweck vollauf erfüllt, wenn sie auf der einen Seite die zentrale Bedeutung des Problems der Uraffekte, auf der anderen Seite seine Verästelungen bis in die höchsten Äusserungen des menschlichen Seelen- und Geisteslebens nachgewiesen haben. Denn wenn die seelisch-geistige Entwicklung nichts anderes ist als eine richtig verstandene Sublimierung der Affekte durch Überwiegen der Selbsthingabe über die Selbstbehauptung und eine damit verbundene Ablösung des Merkens vom Wirken, so sind zwar Apollon und Dionysos, Geist und Seele polare Gegensätze wie Krone und Wurzel, aber, ebensowenig wie diese, Gegner oder Widersacher, die sich bekämpfen oder vernichten müssten. Man könnte vielmehr mit Fug das Wort Kants variieren: Ohne den Geist ist die Seele blind, so wie der Geist ohne die Seele leer bleibt.

GRAPHOLOGICAL EXPERIMENTS WITH THE SCRIPTOCHRONOGRAPH

BY

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The scriptochronograph is an apparatus which enables us to analyse in a simple way and with any exactitude required the temporal relations in writing [1]). Chemically prepared writing paper should be used and a steel nib, transmitting the electric current by which chemical changes in the surface of the writing paper are brought about, and the writing becomes visible. The circuit is interrupted at regular intervals; in the experiments described hereafter as a rule twenty times every second. Therefore every single dot or dash, the composing elements of the written words, represents a certain space of time, in our case one twentieth of a second.

The scriptochronograph however only enables us to make a temporal analysis of those proceedings, during which the nib moves along the writing paper, not of those occurring above the paper, which, in many respects, may be equally important, for instance the time it takes for the nib to move from one word to another of from a word to a diacritical mark (dots on the "i", the double dots for the German "Umlaut"). Neither is the scriptochronograph any use for timing those moments, when the nib does rest on the paper but without moving. (Full stops and dots on the "i"). We therefore mounted an additional apparatus, which can be seen on the right hand side of the photo (Fig. 1). It is a Morse telegraph apparatus, adapted to our purpose.

The electric shocks, producing the electro-chemical writing, are transmitted through the electromagnet of the Morse apparatus, so that every electric shock causes a dash to be made on

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[1]) D. Katz, The Scriptochronograph. The Quarterly Journal of Experimental Psychology, 1948.

the paper, moving though the apparatus at a fixed rate. Thus the number of dashes on the paper, equally interspaced, corresponds to the number of dash- or dot-elements, composing the written words. In this manner it is far easier to count the number of dashes and dots than with the scriptochronograph. The

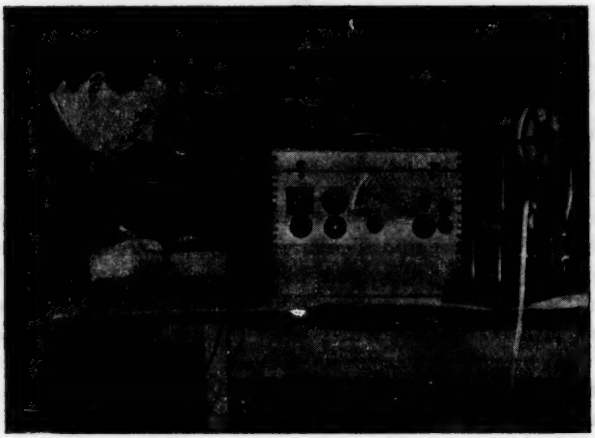

Fig. 1

additional apparatus has the further advantage — as stated before — of giving information regarding many aspects of the production of writing, which escape us when merely employing the scriptochronograph. On the paper moving through the Morse apparatus it is possible to measure the time pauses between the written words or between part of words, whenever an interruption in the act of writing occurs and furthermore all pauses required for the making of diacritical marks or full stops.

In the following paragraphs a description is given of some time analyses of various handwritings.

In fig. 2 are shown some examples of the manner in which the temporal analyses are carried out. It contains the scriptochronograms of the letters i, e, a and ä, as well as the time values as measured with the additional apparatus. There were 20 dashes to the second.

i: 14 points according to Morse's apparatus.
The movement towards the dot on the i took a remarkably long time, viz, about $^{16}/_{20}$ sec., the i-dot itself $^{2}/_{20}$ sec.
e: 13 points according to Morse's apparatus.
a: 16 points according to Morse's apparatus.
ä: 14 points for writing the a. The movement towards the double lines of the Umlaut took about $^{8}/_{20}$ sec., the first of these lines $^{2}/_{20}$ sec., the second $^{4}/_{20}$ sec. The interval between these two was about $^{6}/_{20}$ sec.

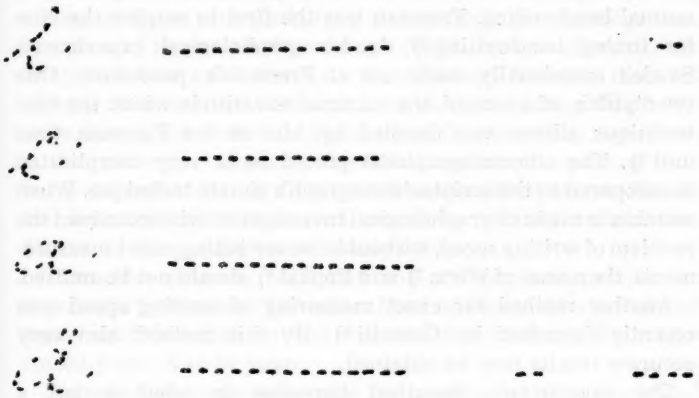

Fig. 2

The construction of the scriptochronograph together with the additional apparatus gave us a very useful instrumentarium indeed. By means of this instrumentarium we were able to solve a problem which has occupied many investigators ever since graphology became a scientific discipline, i.e. the measuring of what Klages calls the absolute or objective writing speed [2]). The use of the scriptochronograph however is not limited to examining the writing speed. It can also be employed for the exact analysis of several other important graphological moments in connection with the time factor, as e.g. the interrelations of tension phenomena, the motor and inhibitory impulses, the rhythm of performance and its disturbances (writer's cramp, tremor, ataxia).

If in former experimental investigations the problem of wri-

[2]) L. Klages, Handschrift und Charakter, 21. u. 22. Aufl., Leipzig '40.

ting speed was touched upon at all, the best result obtainable was no more than a rough estimate. For this reason Wittlich wrote recently: "It is not possible to ascertain from the handwriting its absolute writing speed. It can only be deduced by attempting to gain an impression of the rapidity of writing" [3]. Using Edison's electrical pen Binet and Courtier were the first to measure writing speed exactly [4]. The pen used by them is constructed in such a manner, that holes are made on the paper on which it writes, viz. 11,000 a minute. According to Binet and Courtier's own description it is hardly suitable for producing normal handwriting. Freeman was the first to employ the film for timing handwriting [5]. In his graphological experiments Saudek occasionally made use of Freeman's procedure. One twentyfifth of a second, the maximal exactitude which the film technique allows, was denoted by him as the Freeman time unit [6]. The cinematographical procedure is very complicated as compared to the scriptochronograph's simple technique. When mention is made of graphological investigators who examined the problem of writing speed, without however taking exact measurements, the names of Wirtz [7] and Pophal [8] should not be omitted.

Another method for exact measuring of writing speed was recently described by Gemelli [9]. By this method also very accurate results may be obtained.

The experiments described hereafter in what is but a preliminary report were made for measuring the objective (absolute) writing speed in relation to the relative writing speed. According to the customary graphological terminology the relative writing speed signifies the time covering the entire production of a sample of handwriting. We shall call it hereafter

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[3] B. Wittlich, Angewandte Graphologie. Berlin 1948.

[4] A. Binet et S. Courtier, Sur la vitesse des mouvements graphiques. Revue philosophique, 353, 1893.

[5] Frank Nugent Freeman, An experimental analysis of the writing movement, Psychological Monographs, 17 1914.

[6] R. Saudek, Experimentelle Graphologie. Berlin 1929.

[7] I. Wirtz, Druck und Geschwindigkeitsverlauf von ganzheitlichen Schreibbewegungsweizen. München 1938.

[8] R. Pophal, Grundlegung der bewegungsphysiologischen Graphologie. Leipzig 1939.

[9] A. Gemelli, Contributo all' analisi dei movimenti della scrittura. Commentationes. Pontificia Academia Scientiarum 12, 1948.

the *total writing time*. The absolute (objective) writing speed applies to all instances during which the writer's nib is in contact with the paper and in motion. For brevity's sake we shall call it hereafter the *objective time*.

Apart from the objective time the total writing time includes those instances during which the steel nib is taken off the paper, as e.g. for pauses between the words, and respective sentences, for interruptions between the letters and in the letters, for immaterial curves as e.g. the diacritical marks, for resting pauses on the paper and for reflection pauses. The total writing time was measured by us with the help of a stopwatch, the objective time by means of the scriptochronograph. Not to make matters unduly complicated we shall designate the difference between the total writing time and the objective time as the *"pausing time"*. We were mainly interested in discovering the individual variations in the relation between the objective time and the pausing time in regard to the total writing time. We also wished to see whether and how this relation was influenced by varying certain conditions in the production of handwriting.

Thirtyfive subjects, both men and women, took part in the experiments described hereafter. Most of them had a university degree and therefore writing was no effort to them. Their ages varied from 18 to 50 years.

We asked all subjects to write the samples, which we intended to examine with the scriptochronograph, with pen and ink in the usual way and then only with the scriptochronograph. With the exception of one sample, which had to be written at top speed, all the others were executed at the writers' own natural speed.

Every subject was asked to write the following items in the order given, viz.: 1. name, date of birth, profession, 2. two lines to be written by heart, taken ad libitum from a text, which the subject however was required to choose beforehand, 3. a text to be copied out, composed of several lines containing 50 words of together 209 letters. The subject was allowed to read this text first as often as he wished. 4. The numerals 0, 1, 2, 3 etc to 9, and numbers, composed of several numerals, containing 27 numerals altogether.

The sample sub 2 had first to be written at normal speed (*a*) and then with the following variations: (*b*) at top speed, (*c*) very

large, (*d*) very small, (*e*) with twisted handwriting. A report on the results of this last variation which was made in view of graphological expertise will be published separately.

Before starting the experiment every subject was given the opportunity to try out writing with the scriptochronograph and to get used to the steel nib without being disturbed by the elastic electric cord attached to it.

In the following paragraphs we shall restrict ourselves to examining the results obtained with tests Nos. 2, 3 and 4, beginning with test No. 3. As was to be expected there were considerable individual variations both in the total writing times and in the objective times. The total writing times varied from 108 to 244 seconds with an average of 138.1 seconds. The objective time varied from 48.3 to 128.5 seconds with an average of 75.6 seconds. These results show that out of the total writing time a space of 54.7 % is taken up by what we call "nib on paper" and as much as 45.3 % by the pausing time. This latter percentage is larger than seemed likely according to general considerations and basing ourselves on our own impressions when writing. In test No. 4 (numerals) the pausing time percentage is even larger, viz. 47.7 %. Here the total writing times amounted to between 15 and 30 seconds with an average of 21.4 seconds, and the objective times to 8.0 and 18.1 seconds with an average of 11.2 seconds.

Apparently all 35 subjects participating in the experiment can be divided into 4 groups, viz.:

1. those with a short total writing time together with a relatively short objective time. Both times are below the average. This group therefore shows a great speed in writing.
2. those who combine an average total writing time with a none too short objective time. This group therefore writes with very short pauses.
3. those who show a somewhat longer total writing time than the foregoing but with a quite remarkably short objective time. The steel nib is on the paper for a very short time only, but very long pauses are made.
4. those with a very long total writing time together with a very long objective time. The people in this group write slowly and make correspondingly long pauses.

In the following schedule the reader will find some of the more remarkable results of the four groups we just described.

Group	total writing time	objective time	nib on paper	pauses
1	116.5 seconds	65.0 sec.	55.8 %	44.2 %
2	131.5 „	91.8 „	69.8 %	30.2 %
3	144.0 „	51.7 „	35.9 %	64.1 %
4	197.5 „	103.6 „	52.5 %	47.5 %

By far the greater number of subjects are to be found in groups 1 and 4. The groups 2 and 3 refer to exceptional cases as may be clearly seen, when a comparison is made with the average results given below:

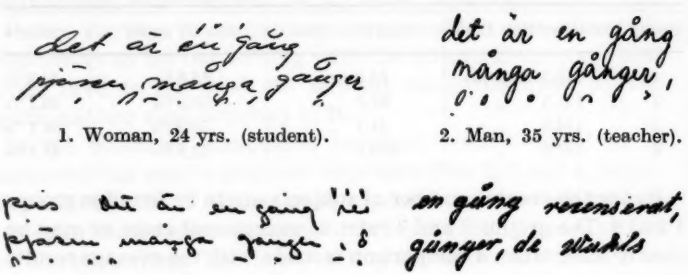
total writing time	objective time	nib on paper	pauses
138.1 seconds	75.6 sec.	54.7 %	45.3 %

Until now graphologists were wont to look for certain characteristics in order to estimate the absolute writing speed. These characteristics were held to indicate speed or slowness. Saudek e.g. in his work quoted on a previous page gives 8 primary and 4 secondary characteristics for speed and slowness and 5 ambiguous characteristics.

Klages in his work quoted before enumerates 9 of these characteristics. Of course it was not possible for them to discriminate between the writing speed of "nib on paper" and "nib above paper". Both authorities realise that they were unable to fully analyse the process of writing. In his work already quoted Klages raises this question: "Does the impression of haste always imply the greater, the impression of slowness the lesser objective (absolute) writing speed?" He gives the following answer: "The problem is still being investigated, but this much can be said. In the majority of cases there is indeed some sort of correspondence. In the minority there is not. We are however unable to give the exact percentage."

Wittlich in his work quoted before is of the same opinion. "A handwriting may give the impression of having been executed rapidly without the writer having written with great speed, or it may not give the impression of rapidity and yet have been

very quickly executed. However as a rule the characteristics of haste and rapidity and leisure and slowness coincide."



1. Woman, 24 yrs. (student). 2. Man, 35 yrs. (teacher).

3. Woman, 37 yrs. (teacher). 4. Woman, 19 yrs. (household assistant.)

Fig. 3. Examples of the four types of handwriting.

Our scriptochronograph puts an end to the unsatisfactory condition of a merely subjective estimate of hurry or leisure. With its help we are now able to decide whether the characteristics of Saudek and Klages are really appropriate. It will clearly demonstrate which characteristics of speed can be traced in the absolute quickest and in the relatively slowest handwriting and whether a subdivision of the characteristics is really necessary. The same applies to be characteristics of slowness.

So far the results of tests 3 and 4 have been discussed. We will now look at the results of test 2. Here the test was not copied out as in tests 3 and 4, but written spontaneously. It seemed likely that the total writing time of a spontaneously written text would be shorter than that of a copied one, because in writing spontaneously the impulses proceed more smoothly than when they are also being determined by an external example. In comparison to test 3 the handwriting in test 2 not only shows greater speed, but it also shows a change in the relation between "time on paper" and the pauses. Whereas test 3 showed percentages of respectively 54.7 and 45.3 those of test 2 shift to 64.6 and 35.4, i.e. to the disadvantage of the pauses. It is comprehensible that in spontaneously writing the hand adapts itself more quickly to the next word than in copying out an example.

When the example of test 2 had to be executed as fast as possible (2b) the total writing time decreased by 21,3 %, the

objective time by a mere 8 %, which is not considerable. A spectator or participant in this sort of speed writing would expect a far higher percentage. If we may be allowed to draw a general conclusion from this special result we would like to point out to mental hygienists that the gain obtained by a maximum effort bears no sensible relation to the extra amount of psychophysical energy spent on making it. The percentages for objective time and pauses were 72.6 and 27.4. The accelerated writing therefore was made more at the expense of the pauses than of the objective time. Probably one would obtain a similar result, if one would make a parallel test for speaking at a normal and at an accelerated rate.

In the 2c case, where the subjects were instructed to greatly enlarge their handwriting the percentages for objective time and pauses were resp. 72.6 and 27.4. The exact concurrence of these values with those of the former test is of course a rare coincidence.

The last variation of test 2 (2d) was an instruction to greatly reduce the size of the handwriting. Here the respective percentages were 64 and 36. The shifting in favor of the pauses compared with 2c is probably due to the fact that it is more difficult to reduce than to enlarge the size of one's handwriting, so that the adaptation to the next word is delayed.

The instructions for cases 2c and 2d (enlarged and reduced writing) were not defined more exactly and the surprising fact remains that none of the subjects made any enquiries as to their exact interpretation. It also is quite remarkable from a graphological point of view that whilst several subjects enlarged their handwriting five times, a few others only did so one and a half time. As was to be expected the variations in the reduced handwriting were not so excessive, but they nevertheless lay between one half and one third of the normal size. It ought to be a matter of no mean graphological interest to investigate the causes of these considerable individual differences.

PSYCHOLOGY AND EVOLUTION

by

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In the psychology of human perception it is customary to distinguish between Empiristic and Nativistic interpretations of given facts. What do these terms mean? The answer seems obvious: an Empirist favors explanations of perceptual facts in terms of learning, while a Nativist prefers explanations in terms of inherited mechanisms which are located in the nervous system [1]). If there are any major difficulties in the Empirist's way of theorizing, such difficulties will not be discussed in these pages. Rather, I propose to analyze what is commonly supposed to be the Nativist's program; because the characterization of this program which I have just mentioned, and which would probably be regarded as adequate by many psychologists, seems to me to be most seriously misleading.

The main point is this. When referring to inherited particularities of the nervous system, we clearly mean *histological* facts, even if in a given case we may be unable to indicate precisely what these facts are. Thus, when it appears that a certain phenomenon in perception is not brought about by learning, we conclude that it is such histological conditions which

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[1]) The term "Empirist" may sound unusual to English and American psychologists. But we need a special name for theorists who tend to give bewildering facts in perception an interpretation in terms of learning. The word "Empiricist" does not refer to such theorists. A philosopher of the Empiricist school might actually prefer Nativistic to Empiristic theories of perception; his principles do not compel him to prefer the latter. Again, philosophers who are Rationalists rather than Empiricists often reason in the Empiristic fashion when they deal with problems of perception. We should therefore confuse two different issues if we were to use the name „Empiricist" both when we mean a particular school in epistemology and when we talk of what I prefer to call the Empiristic trend in the explanation of perceptual facts.

are responsible for its occurrence. I should like to show that this inference is by no means generally justified. A phenomenon in perception which is unlearned need not, for this reason, depend upon the existence of special histological factors. Nativism is a dangerous term; it covers several theoretical possibilities. Unfortunately, we are for the most part aware only of the one toward which the expression "inherited" points. It is important that we know what other factors may be involved in unlearned function. For our issue is by no means restricted to a form of theorizing in perception. Even the intellectual life of man will easily be given a wrong interpretation if we do not realize that Nativistic theories tend to include a far too special premise.

At the present time, all biologists agree that, if a nervous system exhibits certain histological conditions, the cells of the species (and, in particulars, those of the individual) are ultimately responsible for the fact. It is the chromosomic equipment of the cells which has forced morphogenetic processes to establish those structures. The chromosomic equipment of a species, on the other hand, is assumed to be a product of evolution. Hence, if a theorist gives us a Nativistic explanation of certain phenomena, and if his theory is of the kind to which the name "Nativistic" commonly refers, he always assumes that, in the last analysis, those phenomena are made possible by particular achievements of evolution.

It seems to me that this procedure is incompatible with the very meaning of evolution. For, the principle of evolution implies that all biological events, including the functions of the brain, have *some* characteristics on which evolution has never had any influence whatsoever.

In its most consistent form, the postulate of evolution maintains that once the behavior of the inanimate world is sufficiently known it must be possible to derive all biological facts from principles which hold for non-living systems. At the present time, nobody can be forced to accept this radical postulate. I will nevertheless adopt it for my present purpose, because in this fashion my task will be greatly simplified. It follows, of course, that I cannot try to convince Vitalists or those who believe in Emergent Evolution. In a sense, this does not affect my argument. The error which I propose to discuss is

often made by authors who regard the postulate as a necessary part of modern scientific thinking.

Evolution is commonly regarded as a principle of *change* or *development*, and this is also the natural meaning of the term. None the less, the postulate which I have just mentioned is a postulate of *invariance*. It does not indicate what actually happens when organisms develop from inanimate systems, or when new species arise. Rather, it states that, however such changes may be brought about, the same principles as hold in physics and chemistry apply also to these transformations, and to the forms of life which thus originate. Of course, those who agree with this thesis will readily admit that present formulations of those principles may have to be modified when attempts are made to apply them to living systems. General formulations which excellently serve the physicist's purposes may not explicitly refer to situations which play a particularly important rôle in biology. In biological theory, principles of science may therefore have to be stated in a way to which the physicist is not accustomed. It goes without saying that reformulations of this kind are entirely compatible with the postulate of invariance in evolution.

When referring to principles, I have in mind such general propositions as the First and the Second Laws of Thermodynamics, but also the Law of Dynamic Direction, a law which is implied rather than explicitly formulated in physics [2]). But the postulate of invariance applies not only to general principles. It also demands that no forces and elementary processes occur in organisms which do not also occur in physics and chemistry. By forces I mean such vectors as electric and gravitational fields. Examples of elementary processes are electric currents and currents of diffusion. It does not, of course, follow from the postulate that all forces and elementary processes which are known in physics must also play a part in living systems. What is meant is merely that such forces and processes as are actually encountered in organisms invariably have counterparts in the inanimate world. It will be realized that all concepts to which the postulate of invariance refers, namely, general principles,

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[2]) Cf. W. Köhler, *The Place of Value in a World of Facts*. New York, 1938; pp. 306 ff.

forces, and elementary processes, are concerned with *action*. Obviously, as I am now using this word, it applies not only to events which involve changes but also to steady states [3]).

If so much is supposed to have remained invariant in evolution, what can have varied while evolution took place? There must be factors in nature which can change irrespective of the fact that the general principles, the forces, and the elementary processes of all action remain the same. Any textbook of physics can tell us what these factors are. The same principles apply, the same forces operate, and the same processes occur under conditions which vary widely from one system to another. Take mechanics, the discipline which deals with the movements of objects. It is a form of action that objects in the neighborhood of our planet tend to approach its surface. But objects may either be free to follow the direction of the gravitational vector, or given conditions may restrict this freedom. When placed on an oblique plane which is rigid and solid, an object still approaches the earth, but it does so in the direction of the plane and more slowly; because the resistance of the plane eliminates the component of gravitation which would operate at right angles to the plane, and only the component parallel to the plane accelerates the object. Given conditions which exclude certain possibilities of action are called *constraints*. The mechanics of solid objects is not the only part of physics in which constraints modify action. If a gas is surrounded by the firm walls of a container, these walls are constraints. Many processes can occur in the gas, but all those are prevented from taking place which would involve a displacement of the walls, and thus the gas cannot expand as it would otherwise do. In hydrodynamics, a rigid tube in which a liquid is enclosed is obviously a constraint; in contact with the inner surface of the tube, the liquid can move only in the direction of the surface. It is perhaps not customary to use the same term in the case of electric phenomena; actually, however, when a non-conducting substance surrounds a material in which electric currents spread, this substance plays the part of a constraint.

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[3]) I am aware of the fact that, in physics, the term action has also a much more technical meaning. In the present connection, we are not concerned with action in this sense.

No constraint in the sense in which we have just used the concept makes a positive contribution to the action upon which it is imposed. In this sense (although not in others) the rôle of such constraints is negative. They serve to exclude certain actions which would be possible if the constraints were not present. But while in this fashion some components of forces and of elementary processes are eliminated, the remaining components do not change their behavior. The laws which hold for forces and processes are formulated in general terms so that, when certain possibilities of action can no longer be realized, the same laws still apply to such actions as are not prevented by constraints. On an oblique plane, for instance, the component of gravitation which operates in the direction of the plane accelerates an object in precisely the same way as it would if the constraint were absent. The general principle which is here involved is the principle of the conservation of energy. On the oblique plane, increments of kinetic energy and losses of potential energy are smaller for a given period than they would be in the absence of the plane. Since both changes are of the same size, the principle holds in this situation just as it does in the case of free fall [4]). Similar considerations apply to the other instances of action under constraint which have been mentioned in the preceding paragraph.

We can now return to our discussion of evolution. While the general postulate of invariance in evolution claims that no essentially new kind of action appears in living systems, it imposes no limits upon the constraints which may develop when certain inanimate systems assume the characteristics of organisms, and when the various species acquire their distinguishing traits. In this respect, the postulate demands only that such constraints be established in a way which is compatible with the laws of physics and chemistry. In organisms, many different forms of action are, of course, combined (and mutually interrelated) which seldom occur together in the simpler systems commonly studied by physicists and chemists. Apart from this peculiarity of life, it can, according to our postulate, be *only*

[4]) I am, of course, assuming that the influence of friction can be ignored. If this influence is not negligible, the energy balance of the system becomes more complicated; but the principle of the conservation of energy still remains valid.

specific constraints by which the living world has been made possible. No examples of such constraints will here be needed since most histological structures may be considered from this point of view. But, although the world of living creatures would not exist if evolution had not introduced these structures, action in the organisms can never be explained solely by the constraints to which it is subjected. Constraints alone, I repeat, never cause any action; they merely serve to modify actions which, as such, owe nothing to constraints. Thus, if our general postulate is accepted, any action in any organism involves the operation of factors which are entirely independent of evolution. We have seen that these factors are the forces and the elementary processes of nature, and such more general facts as are formulated in the principles of science.

Just as to any other biological processes, our reasoning must be applied to the cortical events on which the characteristics of mental facts depend. Generally speaking, cortical action is also modified by constraints, and to this extent evolution is partly responsible for the way in which this action occurs. But, *qua* action, it can never be understood only in such terms. For all action is also a matter of processes which evolution has not affected, and which are now not affected by its products, the histological conditions found in nervous systems.

Since this argument may be too abstract to carry full conviction, I will give a simple example. It has recently been suggested that the processes underlying organized perception are steady electric currents which spread in the brain as a continuous medium. If this should prove to be true, the distribution of such currents would partly be determined by histological circumstances which evolution has established in the tissue. Quite irrespective of such special conditions, however, the currents would also follow the general laws which hold for any electric flow in any resisting medium, and cannot have been altered by evolution. If actually some other process plays the part which has just been ascribed to electric currents, our argument must be applied to this other action.

It will now be apparent why we cannot accept the statement that the explanation of all unlearned perceptual facts has to be given in terms of histological conditions. The statement cannot be entirely correct for any perceptual fact. It is only another form

of the same mistake if all unlearned functions are attributed to achievements of evolution. For, quite apart from such achievements, unlearned functions are bound to exhibit certain characteristics which they share with actions in the inanimate world.

Although our argument is so simple that it may almost appear as banal, few discussions in which Nativistic explanations are being considered take account of the fact that when we deal with unlearned functions we must always distinguish between action and its constraints. All authors refer to histological conditions upon which such functions depend, and thus, indirectly, to evolution; but few seem to realize that any brain function whatsoever is also an example of actions which do not, as such, depend upon such conditions. One cannot play Hamlet without the Prince of Denmark; on our stage, however, we are consistently trying to do so.

The present issue is now becoming particularly important because the belief in the omnipotence of learning, which characterized an earlier period in the development of psychology, is rapidly beginning to weaken under the impact of evidence which points in the opposite direction. A few years ago, a Symposium on Heredity and Environment showed the change of trend in a most impressive fashion [5]). It seems, however, that the factors involved in unlearned function which owe nothing to evolution, and can therefore hardly be called "inherited", were not explicitly discussed at this conference. We do tend to ignore these factors. In an excellent article on certain phenomena in human perception, a psychologist recently explained that these facts can be interpreted either in the Empiristic fashion or as consequences of the chromosomic equipment of man. Obviously, the chromosomic equipment of man cannot be made responsible for characteristics which action exhibits in inanimate systems as well as in organisms. I do not believe that the author is a Vitalist; most probably, he would accept our general postulate of invariance. Nevertheless, action and the principles which it follows were not mentioned in his paper. It is quite true that in the determination of all events which we study in human perception some inherited conditions

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[5]) *Symposium on Heredity and Environment.* Psychol. Rev., vol. 54, pp. 297—352; 1947.

and ultimately the constitution of our chromosomes must play a certain rôle. But they can do so only by influencing processes, the nature of which has not been altered in the least while evolution took its course, and while human chromosomes originated. Consequently, all facts in human perception have certain characteristics which are unrelated to the specific make-up of human cells. In this respect, only one point is open to debate. *To what degree* are constraints imposed upon physical actions when these actions occur in human brains?

Our distinction has to be made quite generally, not only when problems in perception are being considered. In fact, if the distinction is ignored in certain other parts of psychology, the consequences are actually much more important. In some cases, they may affect our very concept of man, and may thus have repercussions even in philosophy. In the early years of this century, a Naturalistic conception of man was strongly recommended by some philosophers. One topic to which they applied their program was human thinking. More specifically, they maintained that knowledge and the attempts to widen its scope are not primarily concerned with objective cognition. Rather, thinking was to be regarded as a particular mechanism which evolution has developed in man, and which helps his species to survive. Now, human thinking which is objectively adequate can hardly fail to have useful consequences; but it does not follow that the usefulness of thinking in a particular environment is its most fundamental characteristic, and that its adequacy, when it is adequate, must be defined in terms of its usefulness. For again, although human thought may to a degree be influenced by special conditions which evolution has established, it is, first of all, a form of action. Consequently, the notion that only such conditions, *i.e.*, histological factors, are responsible for the characteristics of thinking cannot possibly be defended. Thinking no less than perception must also follow principles which are unrelated to the particular circumstances of human life, to evolution, and to histological devices. I sometimes wonder what those philosophers meant by nature when they demanded that man be understood in Naturalistic terms. Surely, they ignored the most important of these terms.

In the meantime, their views have had an immense influence upon the intellectual and emotional climate of our historical

period. Generally speaking, there is an optimistic trend in evolutionary reasoning. The changes which have occurred since life first appeared on this planet are commonly regarded as improvements. From this point of view, there is, of course, a great temptation to regard human thinking as the very greatest among all evolutionary achievements, and on this basis to feel more optimistic than ever. The Naturalists did not make this mistake. They realized, and sometimes apparently with a certain satisfaction, that in the evolutionary explanation of human thinking as a useful tool this thinking is actually devalued. There is general agreement among the biologists that of all imaginable devices which might have arisen in evolution those have become stable characteristics of a species which serve to make this species better adapted to its environment. Now, although in a way all adaptation implies improvement, it also has its less attractive phase. To the extent to which the various functions in animals and man are thus usefully conditioned, their value must be regarded as relative to the particular evironment in which the adaptation has occurred. This is precisely what the Naturalists actually meant, or what their followers understood them to mean, when they said that human thinking is a product of evolution. When we now refer to "human nature", we use the expression with an unmistakable accent on the adjective. We seem to mean a quite particular part of nature, the one which is merely human, or human in a restrictive sense. This is the point at which evolutionary optimism has turned sour; at which evolution has become a powerful source of the relativistic defeatism from which our intellectual culture is suffering. And yet, if we follow the principle of invariance in evolution, there is no cause for this particular form of our ailment. When man is thinking, he invariably follows, at least in part, *some* principles of action which hold everywhere, and can therefore not be suspected of being merely relative to his particular environment. To be sure, he would not exist at all, if evolution had not occurred. At present, he would have little to think about if evolution had not given him sense organs, properly conducting nerve fibers, and many other particular devices. But even a modest nerve impulse which travels along one of those fibers obeys some general principles of action no less than the constraints to which it is subjected. Similarly,

man's thinking must have some characteristics which are *not* in any sense determined by evolution and, for this reason, significant only in a relative sense.

In some instances, human thinking may be strongly affected by inherited conditions; in others, it may follow mainly general principles of action rather than such conditions. I have a suspicion that the latter alternative is often realized when human beings grasp relations between objects, and when they derive further relations from those which are given. If this were true, there would still remain unanswered questions as to the cognitive significance of such events. But whatever this significance might be, it would not be limited by the fact that it happens to be human beings in whom the events occur. Moreover, such processes would probably show a certain affinity to the facts which man observes in nature; because, as to certain fundamentals, he would find in such observations what he can also find in himself.

How would a human being be impressed by principles of action if he became aware of them as principles of his thinking? Factual generality, even if it is absolute, need not be related to such concepts as being valid or evident; but it might be so related in the present case. Man might find those principles necessary in the sense in which certain formal principles actually appear to us necessary when we think. Naturally, he would not feel that the recognition of such principles presupposes observation of any particular facts. Rather, their necessity would seem to him to have an *a priori* character. And yet, no subjectivism would follow; because, as I said before, for excellent reasons an *a priori* of this kind would tend to fit empirical evidence.

Possibilities such as these fairly obtrude themselves once the postulate of invariance in evolution has been understood and accepted. Even so, it remains to be seen whether principles of action in nature can really be recognized in the way in which human thinking proceeds. Obviously, it attempts in this direction should end in failure, the postulate of invariance could no longer be accepted in its radical form, and a Dualistic view of the world would become unavoidable.

A PROPOS DE LA PERMANENCE PHÉNOMÉNALE FAITS ET THEORIES

par

A. MICHOTTE (Louvain)

Au moment d'écrire ces pages que je dédie à mon vieil ami le professeur G. Révész à l'occasion de son soixante-dixième anniversaire, je ne puis m'empêcher de songer aux conclusions de l'article sur „L'âge et le talent" qu'il a publié dans les Miscellanea Psychologica, édités lors de la célébration de mon jubilé professoral en 1947.

Son activité scientifique illustre en effet de manière frappante ce qu'il disait alors: „.... l'homme créateur, avançant en âge, conserve ses dons jusque dans la vieillesse, pour autant toutefois que les troubles corporels et mentaux qui résultent de l'âge lui soient épargnés". Et je souhaite ardemment qu'il puisse continuer pendant de nombreuses années encore à apporter aux développements de la psychologie, le précieux appui de son expérience et de ses talents.

Sa production a été si étonnamment abondante et variée qu'il serait difficile de trouver un sujet d'étude qui ne puisse se rattacher à son oeuvre. Le présent travail en fournira d'ailleurs la preuve, car il touche en plus d'un point, au problème de la constance des couleurs et à celui de l'éclairement que les recherches expérimentales poursuivies en collaboration, dès 1907, par Révész et par Katz, ont largement contribué à résoudre.

Le problème de la permanence des choses appartient à ce groupe d'énigmes qui ont vivement intéressé les philosophes empiristes du XVIIIème siècle, les associationnistes du XIXème et quelques autres penseurs d'envergure comme Bergson par exemple, mais dont les résonnances chez les psychologues „scientifiques" ont été bien faibles.

Et c'est assez récemment en somme que des travaux systématiques ont été entrepris à son propos. Rappelons parmi les plus importants, les célèbres observations de Piaget et de ses élèves au point de vue génétique, et quelques recherches expérimen-

tales de Metzger, de Ternus, de Lindemann, de Sampaïo, de Knops, etc. [1]).

On peut se demander quelle est la raison de cette défaveur. Est-ce peut-être parce qu'on situait ce problème plutôt dans le domaine de l'épistémologie et qu'on le croyait peu susceptible de se prêter à l'expérimentation, ou encore parce qu'on se figurait que les lois des phénomènes de „constance" représentaient la principale contribution que la psychologie de la perception pouvait apporter à sa solution?

Mais à vrai dire, dans quelle mesure s'agit-il d'un problème de perception? Et que faut-il entendre au juste par permanence phénoménale?

On ne saurait assez insister lorsqu'on s'occupe de questions de ce genre, sur la distinction qu'il y a lieu de faire entre: „connaissances acquises" et „apparences" [2]).

Ainsi, pour ce qui concerne les problèmes de la permanence et de la non-permanence, l'élaboration des données complexes de l'expérience aboutit naturellement à nous faire „savoir" ou „croire" que certaines choses existaient déjà avant que nous les percevions (ou qu'elles continuent à exister après que nous avons cessé de les percevoir), tandis que d'autres choses n'ont qu'une existence éphémère dont la durée coïncide avec celle de leur perception. Mais cette „croyance" n'est pas tout. Un simple exemple suffira à le montrer.

Tout le monde a vu, au cours de séances de prestidigitation l'opérateur faire apparaître tout à coup, ou disparaître subitement des choses dont on sait pertinemment qu'elles n'ont été ni créées ni anéanties à l'instant. Et néanmoins il est incontestable que l'on a parfois dans ces conditions une impression visuelle évidente de leur naissance ou de leur évanouissement.

Cette création et cet anéantissement ne sont, bien entendu,

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[1]) On trouvera des indications détaillées à ce propos dans: A. C. Sampaïo. La translation des objets comme facteur de leur permanence phénoménale. Louvain. Warny 1943. 31 p. et: L. Knops. Contribution à l'étude de la „naissance" et de la „permanence" phénoménales dans le champ visuel. Miscellanea Psychologica Albert Michotte. pp. 562—610 Editions de l'Institut Supérieur de Philosophie. Louvain. 1947.

[2]) A. Michotte. Le caractère de „réalité" des projections cinématographiques. Revue Internationale de Filmologie Cahier n^{0} 1. Paris 1948. p. 4.

que des illusions, mais il n'y en a pas moins là un problème psychologique; et celui-ci se ramène en dernière analyse à un problème perceptif, car la production de ces phénomènes dépend d'un certain type d'évolution des excitations visuelles qu'il appartient à la psychologie de la perception d'analyser.

Quant à la permanence apparente, les choses sont moins simples et dans certains cas même, il semble inconcevable qu'elle puisse se manifester directement dans les données de l'expérience. La comparaison de ce problème à celui des „constances" est très instructif à cet égard.

La question de la constance vise, comme chacun sait, le fait que certaines propriétés des choses, leur couleur, leur grandeur, leur forme ne se modifient pas d'une façon sensible, alors qu'il se produit des différences considérables dans le système des excitations correspondantes: sous l'influence de la nature de l'éclairement pour ce qui est de la couleur; sous l'influence de la distance pour ce qui est de la grandeur; sous l'influence de l'orientation de l'objet dans l'espace par rapport à l'observateur, pour ce qui est de la forme.

Cette *constance* revêt une importance capitale au point de vue de la permanence puisque, grâce au jeu des lois qui la régissent, les caractères propres des choses demeurent apparemment inchangés au cours du temps.

Mais la *permanence* va plus loin encore. On rencontre en effet à chaque instant des situations dans lesquelles une chose demeure pour nous identique „en soi", bien que *nous percevions des changements dans ses propriétés,* tels les cas; d'une barre de fer qui rougit au feu; d'un ballon qui se dilate; ou d'un coussin qu'on aplatit. L'identité de l'objet peut résister même à sa disparition partielle ou totale du champ sensoriel.

Dès lors, il faut savoir ce que peut être ce „quelque chose" qui persiste malgré tout, et qui ne s'identifie pas avec ses propriétés apparentes. Est-il possible que ce „quelque chose" soit donné dans l'expérience? Et ne s'agit-il pas uniquement d'une construction de l'esprit (substance, ou possibilité permanente de sensations) qui ressortit essentiellement au domaine de la *croyance* dont il a été question ci-dessus?

C'est d'ailleurs bien ainsi que les philosophes envisageaient la question, et c'est pourquoi ils se préoccupaient surtout de déterminer l'origine de ladite croyance. Celle-ci était amorcée

et justifiée, pensaient-ils par certains *indices* fournis par l'expérience: la recognition, les liens associatifs, les „besoins", les manifestations de la motricité, etc. Et tout cela intervient sans aucun doute en l'occurrence. Mais est-il bien sûr que cette permanence résistant aux altérations ou à la disparition de l'objet soit *uniquement* affaire de „savoir" ou de „croyance", et qu'elle n'existe que sur le plan de la pensée?

Quelque improbable que cela puisse paraître a priori, il semble que les données de l'expérience soient en réalité infiniment plus riches à ce point de vue et plus exhaustives qu'on ne le supposerait. De nombreux faits démontrent en effet que ce genre de permanence se manifeste déjà sous différentes formes dans la sphère phénoménale elle-même, et le but de la présente étude est d'en signaler un certain nombre, et de les discuter.

Avant d'aborder ce point cependant, et afin d'apporter quelque clarté dans l'exposé, nous rapellerons brièvement les diverses éventualités de permanence et de non-permanence que l'on doit envisager, ce qui nous permettra en outre de préciser la terminologie utilisée dans la suite.

Pour ce qui est de la permanence elle-même, on peut en distinguer trois types fondamentaux: la *permanence d'antériorité* qui se caractérise en ce que l'objet ou l'une de ses parties paraît avoir préexisté à sa perception (c'est ce qui se produit lorsque par exemple, une chose apparaît par suite de l'écartement d'un objet qui la cachait au regard).

Le cas inverse, de la *permanence de postériorité,* est celui dans lequel un objet ou l'une de ses parties paraît continuer à exister alors qu'il a cessé d'être visible (parce que, par exemple, un autre objet vient le recouvrir).

Il y a encore la *permanence de continuité* c'est-à-dire le fait qu'une chose semble demeurer elle-même, maintenir son identité foncière pendant toute la durée de sa présence en dépit même des changements apparents qu'elle peut éventuellement subir.

Quant à la non-permanence, il faut distinguer trois cas parallèles:

la *création,* quand un objet semble „naître", „surgir du néant";

l'*annihilation,* quand un objet présent „s'évanouit" sans laisser de traces;

la *substitution* qui se présente lorsqu'un objet semble soudainement remplacé par un autre.

Il va de soi que l'on peut rencontrer différentes combinaisons de ces types d'événements, que la création, par exemple peut être suivie d'une permanence de continuité, et qu'à celle-ci peut faire suite une permanence de postériorité; ou bien que la permanence d'antériorité peut s'allier à une annihilation ultérieure, etc.

Cela étant dit, nous allons procéder à l'examen systématique et critique d'une série d'essais expérimentaux relatifs à la permanence et à la non-permanence des objets.

Le premier exemple qui nous arrêtera est une manifestation de la permanence de continuité qu'il est facile de réaliser de la manière suivante [3]).

On projette sur un écran une surface colorée quelconque, un cercle bleu par exemple; et l'on en modifie brusquement la couleur, la grandeur ou la forme. On peut avoir dans ces conditions l'impression que l'objet *a subi un changement tout en demeurant „lui-même"*; c'est ce même cercle lumineux qui est devenu verdâtre, par exemple, ou qui s'est dilaté, ou qui s'est ovalisé. L'observation est d'ailleurs tellement banale qu'il est inutile d'y insister.

Mais, et c'est ici le point important, cette impression de changement ne se produit que dans certaines limites de variations que l'on peut déterminer expérimentalement. Quand les modifications ont plus d'ampleur, et surtout lorsque plusieurs propriétés de l'objet sont altérées simultanément, l'impression est radicalement différente: on voit apparaître un *nouvel* objet, à l'endroit où se trouvait le premier. Il n'y a plus simple changement mais *substitution*, remplacement instantané de quelque chose par quelque chose d'autre.

Bien qu'ils répondent à des différences graduelles dans le système des excitants, changement et substitution sont donc des événements qualitativement différents, dont le premier

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[3]) Cette expérience, ainsi que d'autres qui seront relatées ultérieurement, a été réalisée en vision monoculaire, au moyen d'un appareil comportant un jeu de miroirs permettant de faire apparaître successivement en un même endroit de l'espace deux objets différents. Un obturateur extrêmement rapide assure la substitution pratiquement instantanée de l'un à l'autre.

est une manifestation de permanence, tandis que le second traduit au contraire une non-permanence.

On serait naturellement tenté de croire que l'impression de changement se justifie par le fait que l'altération perçue est partielle et faible, et que, par conséquent, les aspects de l'objet qui n'ont pas été modifiés constituent le „substrat" de la permanence, et cela semble logique.

Mais alors pourquoi n'en va-t-il pas de même lorsque les changements sont un peu plus marqués ou quand ils se combinent, car, dans l'expérience dont nous nous occupons, l'impression de substitution se produit bien que de nombreux aspects de l'objet demeurent identiques. Elle peut être provoquée par exemple par une altération combinée brillance-grandeur, avec *maintien* de la forme, de la microstructure de la surface, de sa planéité, du caractère film de la couleur, etc., et logiquement parlant il n'y a évidemment aucune raison d'affirmer la permanence dans un cas plutôt que dans dans l'autre! Mais il n'est pas question de logique ici, et l'expérience montre précisément que la permanence peut se présenter sous la forme de phénomènes spécifiques, telle l'impression de changement, dont la production est déterminée par un système d'excitations bien défini.

Il n'en reste pas moins qu'il y a là une énigme psychologique, car l'objet ne nous est présent, semble-t-il que sous l'aspect de ses propriétés et l'on peut se demander dès lors ce que signifie cette identité de l'*objet,* opposée à la modification des dites propriétés. La question est d'autant plus pertinente que les résultats expérimentaux montrent qu'aucune propriété *isolée,* pas plus la forme que la couleur ou la microstructure, ne possède le privilège d'assurer l'identité de l'objet.

En réalité cependant, il suffit de considérer le problème dans le cadre des conceptions actuelles en matière de perception pour entrevoir sa solution.

Ce serait une erreur en effet de considérer les différentes variables: couleur, forme, etc., comme autant de données dissociées et indépendantes. Elles apparaissent au contraire comme *propriétés d'un même objet;* elles sont intégrées en une unité, en un même Tout.

On sait de plus qu'elles ne le sont pas au même titre, que certaines sont plus „importantes", plus „accentuées" que d'autres,

et aussi qu'elles s'influencent mutuellement. Le Tout dont il s'agit comporte donc une véritable hiérarchie, une *organisation interne* relativement complexe.

Il en résulte que „l'objet phénoménal" n'est pas une *somme* de propriétés (pas plus que sa forme géométrique n'est une somme de parties), mais un Tout structuré, une Gestalt, dans laquelle les diverses variables sont intégrées et coordonnées. Ce terme de Gestalt ne cache pas, bien entendu, une „entité" plus ou moins mystérieuse! Il a au contraire un sens bien précis au point de vue phénoménal car il désigne tout simplement le *„cachet spécifique"* qui caractérise l'ensemble comme tel des aspects constitutifs de l'objet; et il n'est que trop évident que tout objet perçu, quelque simple soit-il, (se réduisit-il même à un seul point lumineux) possède un pareil „cachet".

Considérés sous cet angle, les résultats de notre essai deviennent clairement intelligibles.

L'impression de changement se produirait lorsque les altérations de certains aspects de l'objet respectent le „cachet spécifique" de l'ensemble; celle de substitution quand celui-ci s'éclipse et qu'il est remplacé par un autre. La permanence est donc rendue possible en dernière analyse, grâce à l'autonomie relative de la Gestalt par rapport aux variables qu'elle englobe; et il appartient à la recherche expérimentale de déterminer ce qui est essentiel à son maintien et dans quelle mesure elle survit aux modifications que l'on apporte au système des excitations.

Cette situation est analogue à celle, bien connue de tous ceux qui se sont occupés de la perception des formes, et dans laquelle l'impression globale de la forme est conservée bien que celle-ci paraisse plus ou moins altérée; telles les figures que nous voyons comme des carrés ou des cercles „déformés". La seule différence entre les deux cas réside en ce que la Gestalt dont il s'agit ici réalise l'intégration de propriétés *disparates*: forme, couleur, etc.

Ajoutons que cette manière d'envisager la solution du problème permet de répondre à la question que nous avons posée plus haut lorsque nous nous demandions ce que pourrait bien être „ce quelque chose qui persiste malgré tout". On peut dire à présent que ce quelque chose n'est pas uniquement objet de pensée, et ne se réduit pas au concept de substance, mais qu'il existe aussi sur le plan phénoménal comme Gestalt. C'est cette

unité, qui se définit non seulement par la présence de telles ou telles variables, mais essentiellement par *leur intégration et leur ordonnancement au sein d'une structure globale*, et qui constitue ce que nous avons appelé le „cachet spécifique" de l'objet.

Nous emprunterons un second exemple, relatif cette fois aux permanences de priorité et de postériorité, à des recherches déjà partiellement publiées [4]).

On montre à un sujet une figure colorée, un carré rouge p. ex. et l'on fait apparaître ensuite contre ce carré une bande blanche plus étroite, qui s'agrandit progressivement et dans une direction perpendiculaire au côté du carré auquel elle est adjacente.

Lorsque la vitesse et les dimensions sont convenablement choisies, l'observateur ne voit pas la bande naître et croître ensuite comme elle le fait „en réalité", mais il voit une bande rigide, de dimensions fixes, „sortir" du carré, glisser „hors du carré" qui la cachait précédemment. C'est ce que nous avons appelé l'effet Ecran.

Il y a évidemment ici permanence d'antériorité (et de postériorité quand on inverse l'expérience) puisque la bande „sort" de derrière un écran qui la couvrait. La partie „cachée" est donc présente bien qu'on ne la voie pas [5]). Elle est naturellement indéterminée au point de vue de sa grandeur; et au surplus ceci n'est possible, faut-il le dire?, que dans le cadre de la perception d'ensemble et à la condition qu'une partie au moins de l'objet soit visible, comme dans le cas classique de la distinction Figure-Fond.

L'expérience réussit dans cent pour cent des cas; et même lorsque le sujet a été mis au courant de la technique utilisée, il lui est pratiquement impossible de voir la bande se dilater ou s'agrandir!

Il n'est d'ailleurs pas difficile de se rendre compte qu'il *doit* en être ainsi, étant donné que les conditions expérimentales adoptées pour cet essai ont pour effet d'assurer l'appartenance exclusive, au carré, de la frontière qui le sépare de la bande.

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[4]) Voir: Sampaïo et Knops. loc. cit.

[5]) Un cas similaire est celui de l'effet Tunnel décrit jadis par Wertheimer.

C'est-à-dire que ladite frontière est perçue uniquement comme une partie du contour du carré, et que la bande elle-même n'a pas de limite propre dans cette direction. *Elle semble donc se prolonger derrière le carré* (comme le Fond s'étend derrière la Figure).

D'autre part on peut démontrer que les conditions de l'expérience et entre autres cette absence d'une limite propre de la bande, empêchent la production de phénomènes de dilatation de contraction, ou de croissance apparentes de cet objet. Il en résulte que *la grandeur apparente de la bande doit demeurer constante* (rigidité).

Et cependant, il est évident que *la longueur visible de la bande* augmente progressivement.

Il suffit de rapprocher ces trois données pour constater qu'elles constituent les traits essentiels de la structure „sortir de", puisque celle-ci se réduit en dernière analyse au fait que *l'on voit augmenter la longueur visible d'une bande rigide partiellement recouverte par le carré.*

On a signalé déjà [6]) combien les manifestations de l'effet Ecran étaient fréquentes dans la vie courante: ouverture d'une porte, d'un tiroir, des rideaux d'une fenêtre, introduction d'une main dans une poche, disparition d'une auto ou d'une personne au tournant d'une rue, même simple ouverture des paupières, etc. Aussi ne pourrait-on surestimer le rôle immense que cet effet doit jouer au point de vue de la permanence apparente des choses.

Retenons pour le moment que cette seconde expérience et beaucoup d'autres que nous avons réalisées en appliquant les mêmes principes, confirment pleinement les résultats de la première. On constate en effet de nouveau que la permanence se manifeste sous la forme de phénomènes spécifiques qui se traduisent ici par l'impression de „sortir de" (ou son contraire, l'impression „d'entrer dans") dont la production est déterminée par la structure du système d'excitations agissant sur l'observateur.

Un troisième exemple doit nous occuper plus longuement.

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[6]) Cfr. Sampaïo et Knops, loc. cit.

Il nous est fourni par une constatation banale entre toutes et qui, cependant, constitue une réelle énigme quand on y réfléchit un instant.

Il s'agit de l'éventualité suivante. Lorsqu'on se trouve dans une chambre parfaitement obscure, dans sa chambre à coucher pendant la nuit par exemple, et qu'on allume brusquement la lumière, les objets que l'on aperçoit, meubles, tentures etc. semblent évidemment permanents; ils existaient déjà avant que nous les percevions. Ce cas est d'autant plus intéressant que c'est précisément l'opposition entre la permanence des objets et l'intermittence de leur perception qui a le plus généralement intrigué ceux qui se sont occupés du problème.

Notons en passant qu'une autre expérience similaire, consistant à projeter soudain dans une salle obscure une image lumineuse sur un écran, donne un résultat qui semble à première vue absolument différent; l'image paraît créée au moment de son apparition.

Ces observations sont tellement courantes et d'une interprétation apparemment si obvie que l'on ne saisit pas d'emblée la difficulté qui s'y attache. Chacun en effet serait tenté d'y voir la manifestation évidente de notre connaissance de la nature des „choses réelles" et de celle des „images lumineuses". Ce serait donc notre croyance qui serait en jeu en l'occurrence soit directement, soit grâce à une influence qu'elle exercerait sur nos „impressions" sensorielles.

Mais cette opinion se heurte immédiatement au fait que la „réalité" d'un objet qui apparaît brusquement ne suffit nullement à garantir sa permanence, ainsi que nous l'avons signalé à propos du cas du prestidigitateur. Là, il est vrai, on pourrait se montrer quelque peu sceptique, et tenter d'expliquer l'illusion par l'intervention d'autres facteurs: distraction, suggestion, etc. Mais voici un essai qui les élimine et qui donne néanmoins le même résultat.

On fait voir au sujet un carré gris de teinte uniforme, plus ou moins foncée, d'environ dix centimètres de côté, dont il fixe le centre. Ce carré est subitement remplaçé par un autre identique, tant au point de vue de sa brillance qu'à celui de sa microstructure, au milieu duquel se trouve un petit objet réel, une vis par exemple. Et le dispositif est réglé de telle manière que le fond gris ne subisse aucun changement perceptible au

moment de la substitution, et qu'il paraisse donc absolument stable pendant toute la durée de l'observation.

Les effets de cette expérience sont constants; les sujets décrivent ordinairement ce qu'ils ont vu en disant que „l'objet" semble „se former", „venir" de rien, sans que cela signifie en aucune manière qu'il semble „sortir" du fond gris dans le sens où l'on voit un objet „sortir de derrière un rideau". Il n'y a pas de trace de l'effet Ecran ici et l'objet apparaît au contraire comme quelque chose qui „se déploie" soudainement là où il n'y avait rien auparavant. Cette expérience comporte un dynamisme interne, un „devenir" de l'objet qui est évident. Et du reste les bons observateurs ne manquent pas de remarquer la présence de mouvements Gamma (particulièrement accentués pour les objets allongés) dont l'effet est que la „chose" paraît „se développer" à partir du point de fixation [7]).

Il se produit donc une véritable impression de naissance, de création dans le sens obvie du mot, analogue à celle que l'on observe lorsqu'on assiste à l'éclosion d'une flamme ou à celle d'une bulle de savon. Dans un cas comme dans l'autre, il y a ce travail intérieur d'expansion qui, soit dit en passant, paraît bien être essentiel au caractère de „création" dans le domaine visuel.

En tout état de cause, le fait fondamental, sur lequel nous devons insister pour le moment, est que la présence de ce caractère de création, dans cette expérience particulière, est en contradiction patente avec la conviction du sujet que la chose aperçue existe réellement quelque part et qu'en vérité elle „ne sort pas de rien"! Aussi tout adulte normal se rend il compte qu'il est le jouet d'une illusion provoquée par un artifice quelconque.

Mais, si l'on peut ainsi provoquer l'illusion de la création d'un objet que l'on sait permanent, il est possible également de produire une illusion de permanence pour un objet dont on sait qu'il se forme au moment même où on l'aperçoit. Ainsi, il est aisé de conférer une permanence apparente d'antériorité aux images des projections lumineuses dont il était question ci-dessus, en faisant intervenir l'effet Ecran.

On place devant l'objectif du projecteur un morceau de carton

[7]) Cf. Knops, loc. cit. pp. 584 scq.

qui intercepte les rayons lumineux et empêche l'image de se former. On fait alors pivoter lentement le projecteur autour de son axe de façon que les rayons dépassent l'obstacle et atteignent une feuille de verre dépoli. L'image *commence alors à exister*, et elle se déplace, tout en se complétant, au cours de la rotation de la lanterne. Physiquement parlant, il y a donc „création" à ce moment.

On peut expliquer tout cela au sujet, lui faire voir la manière dont fonctionne le dispositif, etc.; mais s'il se place ensuite de l'autre côté du verre dépoli, il verra néanmoins de manière coërcitive l'image „sortir" de derrière quelque chose qui la cachait. Pour l'observateur, c'est „l'image-sur-le-verre-dépoli" elle-même qui glisse hors d'une cachette qui la recouvrait et où par conséquent, elle préexistait!

La conclusion s'impose. L'impression de permanence, comme celle de création ne sont pas nécessairement liées à la nature phénoménale des objets; leur présence est déterminée par les conditions d'excitation, aussi bien dans le cas de choses réelles que dans celui d'images lumineuses.

Ce point étant établi, nous pouvons examiner à présent d'un peu plus près notre exemple de la chambre obscure afin de voir quelle est la raison de la permanence des objets dans ce cas spécial.

A cet effet, on peut réaliser une expérience qui rappelle cette situation.

Le dispositif adopté pour cet essai est le même que celui de l'avant-dernière expérience, à cette différence près que le fond gris de la première phase est supprimé et que le champ visuel est absolument obscur au début (comme dans la chambre). A la seconde phase, apparaissent soudain le carré gris et l'objet placé en son centre.

A l'inverse de ce qui se passait précédemment toute impression de création de l'objet fait ordinairement défaut ici. L'objet semble stabilisé dès l'abord et il n'y a d'ailleurs plus de manifestation nette de mouvements Gamma. Cette première constatation, bien que purement négative, est intéressante en ce qu'elle montre que la *création de l'objet* ne se produit pas nécessairement chaque fois qu'il y a excitation brusque, et que sa présence dépend de facteurs additionnels comme les mouve-

ments Gamma, qui réalisent un véritable „devenir" de l'objet.

On pourrait chercher à déterminer pourquoi les mouvements Gamma sont absents dans le cas présent, et il y aurait beaucoup à dire à ce propos, mais cette question étant accessoire à notre point de vue actuel, nous ne nous y arrêterons pas, nous contentant d'enregistrer le fait comme tel.

Quand aux résultats positifs de l'expérience, ils diffèrent d'après les cas et on peut les classer en deux catégories principales que, pour la facilité de l'exposé nous appellerons A et B.

Catégorie A. Certains sujets se bornent à dire qu'ils „voient apparaître l'objet" et il est assez difficile d'obtenir d'eux des indications plus précises. Toutefois, lorsqu'on insiste, il semble se dégager de leur réponses que leur impression traduit assez adéquatement la situation physique; il y a pour eux simple *succession* d'un stade auquel „l'objet est présent" à un stade „où il n'y avait rien du tout" et le passage de l'un à l'autre n'est apparemment marqué par aucun caractère particulier.

L'observateur est donc entièrement *dominé par l'objet* inerte (absence de „devenir"), dominé de façon si exclusive que l'expansion de la lumière qui se produit régulièrement comme on le sait lors de toute illumination subite d'un champ plus ou moins étendu n'existe pas, „passe inaperçue" pour lui; et dans la mesure où elle existe phénoménalement, elle n'est en tout cas qu'un accessoire sans importance qui ne joue aucun rôle dans l'expérience d'ensemble.

Catégorie B. Ici le tableau est tout différent, et en tous points semblable à ce qui se passe dans le cas de la chambre noire. Les sujets ont l'impression de „l'éclairement soudain d'un objet qui se trouvait déjà là"! C'est dire qu'il y a création de la lumière et permanence d'antériorité de l'objet.

La dissociation bien connue entre l'impression de l'illumination et celle de l'objet (de ses couleurs propres) est donc capitale dans ce cas. La lumière comme telle jouit d'une autonomie phénoménale marquée, et elle apparaît comme l'un des constituants principaux de l'expérience globale.

Au point de vue descriptif, les catégories A et B se différencient donc essentiellement par la structure interne de la perception, par la hiérarchie qui s'établit entre ses composantes.

Ceci étant acquis, différents points demandent à être discutés d'une manière plus approfondie.

En premier lieu, pourquoi n'y a-t-il pas d'impression de „création" dans le cas de la catégorie A?

Il suffit de formuler la question d'une autre manière pour la faire voir sous un jour tout différent. Elle revient en effet, en dernière analyse à se demander pourquoi l'objet ne possède pas de limite temporelle antérieure *propre* dans ce cas.

Il va de soi que l'apparition (la disparition éventuelle de l'objet) se produit à un certain moment du temps phénoménal et qu'elle est donc „située" dans le cours temporel général de la vie psychique. Mais cette détermination demeure extrinsèque à l'objet et marque uniquement le début de sa *„présence"*, sans impliquer aucune indication relativement à son origine, à sa genèse. Celle-ci reste complètement indéterminée; l'objet ne semble *ni créé, ni préexistant*, à l'instant où il commence à faire partie de notre expérience perceptive, et la seule chose que l'on puisse dire à son sujet est précisément ce que rapportent les observateurs lorsqu'ils répondent „qu'il n'y avait rien et que, maintenant, il y a quelque chose", formule banale, mais qui est, au fond d'une exactitude étonnante.

On peut ajouter que cette situation phénoménale est très semblable à celle que l'on rencontre souvent dans les cas de substitutions, tel celui de notre tout premier exemple. Ce serait une erreur en effet de concevoir la substitution comme impliquant nécessairement une „création" succédant à une „annihilation" phénoménales; il arrive fréquemment au contraire qu'elle soit décrite elle aussi au moyen de la formule suivante: „il y avait cela à tel endroit, et maintenant, tout à coup, il y a ceci", formule semblable à la précédente et dont il semble résulter que l'impression typique de la catégorie A se ramène en fin de compte à une pure substitution.

Dans le cas de la „création", il en va tout autrement. Il ne s'agit plus uniquement d'un *début de présence*, mais d'un *commencement d'existence* puisque l'on assiste à la formation de l'objet, à son déploiement à partir de rien! L'objet même subit un développement progressif, on le voit „devenir" ce qu'il est, processus évolutif qui peut se réduire dans les cas extrêmes à de simples mouvements Gamma.

Or, il est évident que l'objet possède dans ces conditions une

structure temporelle intrinsèque correspondant au rythme de son développement, et que la limite antérieure de cette évolution constitue la limite temporelle propre de l'objet. Et celle-ci lui appartient au même titre que dans le domaine spatial, le contour d'une figure appartient à celle-ci et non au Fond sur lequel elle se détache.

Tout le problème se centre donc sur la distinction, qui paraît essentielle en la matière, entre le temps *externe* à l'objet, dans lequel celui-ci est simplement localisé, et son temps *propre*, caractéristique intrinsèque qui assigne une limite définie au début de sa formation, en fonction de celle-ci même et pas seulement en fonction du déroulement de l'ensemble de la vie mentale.

En définitive c'est donc l'absence d'évolution apparente de l'objet (et même de l'illumination) qui semble devoir rendre compte du fait qu'il n'y ait aucune impression de naissance dans le cas de la catégorie A, et cela explique d'une manière générale pourquoi l'apparition subite d'un objet dans notre expérience ne se présente pas toujours sous la forme d'une „création" phénoménale [8]).

Ce qui précède fait aisément comprendre par contre qu'il y ait impression de création de l'illumination dans le cas de la catégorie B. Ceci résulte à la fois de „l'expansion" phénoménale habituelle de la lumière, laquelle constitue une véritable évolution, et de l'autonomie considérable qu'elle possèdait alors par rapport à l'objet [9]).

Mais il y a un autre aspect de la structure B que nous n'avons pas encore touché et qui est important lui aussi. Il nous semble indubitable en effet qu'il y a dans ce cas impression de *préexistence* de l'objet par rapport à son illumination. Et ceci

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[8]) La même chose se vérifie semble-t-il dans le domaine de l'audition. Quand on fait entendre soudain un son dans une chambre silencieuse, il y a également simple succession d'un stade de présence à un stade d'absence. Mais il en va autrement lorsque l'apparition s'accompagne d'une augmentation progressive d'intensité, ou lorsque l'on fait entendre une mélodie qui, par sa nature même, possède un temps propre.

[9]) La dissociation entre l'éclairement et les couleurs propres de l'objet se manifestaits à vrai dire également dans les autres expériences, en ce que l'objet donnait d'emblée l'impression d'être fortement éclairé (dans nos conditions expérimentales). Mais le fait essentiel dans ces cas était l'absence du „devenir" caractéristique de l'illumination.

constitue un nouveau problème étant donné que la préexistence d'antériorité implique une certaine détermination, si vague soit-elle d'ailleurs, de la durée propre de l'objet. Or, celui-ci paraissant inerte, stabilisé, comment se fait-il qu'il présente ce caractère d'extension dans le passé?

La comparaison des catégories A et B fournit peut-être une indication à ce sujet. Si en effet, la préexistence apparente ne se manifeste que pour la catégorie B, c'est qu'elle est liée d'une façon ou de l'autre au caractère qui différencie celle-ci de la catégorie A, c'est-à-dire, à la présence de l'impression de *création de l'illumination.*

Il faut remarquer à ce propos que les rapports de l'objet et de son éclairement ne se réduisent pas à une simple juxtaposition. La lumière *affecte* l'objet puisqu'elle l'illumine et, malgré la distinction de ses différents aspects, l'expérience globale constitue une unité très forte, bien ordonnée, pleine de sens, qui s'oppose à ce point de vue au caractère un peu incohérent de la catégorie A.

Mais si les constituants de l'expérience subjective (objet et éclairement) sont aussi étroitement unis, il n'est pas étonnant que les propriétés de l'un puissent avoir une certaine répercussion sur celles de l'autre. Reste à voir comment cette influence pourrait être conçue.

On pourrait songer entre autres à une interprétation assez simple, qui aurait en outre l'avantage d'établir un nouveau rapprochement entre le domaine du temps et celui de l'espace. Comme nous l'avons signalé il y a un moment, l'éclairement possède une *limite temporelle antérieure propre* puisqu'il semble créé au moment de son apparition, tandis que l'objet n'en possède pas. Or ceci fait naturellement penser encore une fois à la distinction entre Figure-Fond. Là aussi, il s'agit de deux constituants de l'expérience dont l'un possède une limite alors que l'autre n'en a pas. Et le résultat de leur coordination dans l'espace se manifeste en ce que le fond semble se prolonger derrière la figure, ce qui pourrait s'exprimer encore en disant que le fond semble exister en deçà des frontières de la figure. Pourquoi n'en irait-il pas de même pour le temps étant donné que les conditions d'excitation sont parallèles dans les deux domaines? Les deux événements, illumination et apparition de l'objet ont, *physiquement parlant* une limite temporelle commune

puisqu'ils se produisent au même moment du temps physique, comme les deux plages du champ spatial (celle de la Figure et celle du Fond) ont une frontière commune dans l'espace physique. Mais phénoménalement parlant, la limite temporelle n'appartient qu'à l'un des événements, comme la frontière spatiale n'appartient qu'à l'une des plages. Dans ces conditions, on ne voit pas pour quelle raison l'autre constituant de l'expérience globale, l'objet, ne pourrait pas sembler se prolonger en deçà de la limite temporelle de l'illumination et donner par conséquent une impression de préexistence, au même titre que le fond donne l'impression d'être présent derrière la Figure. Toutes les données du problème nous paraissent au contraire plaider en faveur de cette conception.

D'autres hypothèses pourraient également être prises en considération. Ainsi, il ne paraît pas impossible que l'opposition entre le „devenir" de l'éclairement et la stabilité de l'objet se trouve accentuée dans la structure d'ensemble, „comme si" la progression, temporelle de l'illumination entraînait une régression apparente de l'objet, et rejetait celui-ci en quelque sorte dans le „passé", passé indéterminé d'ailleurs et qui se traduirait simplement par une impression de „déjà là".

Pareil phénomène pourrait se comprendre à l'instar de ce que l'on observe dans le cas des mouvements induits. Lorsque deux points forment une unité de groupe et que l'un d'eux se meut dans la direction de l'observateur alors que l'autre demeure immobile, on a dans certaines conditions, comme on le sait, l'impression que celui-ci se déplace en sens opposé. N'y aurait-il pas quelque chose de semblable ici?

Tout ceci ne sont que de simples suggestions, bien entendu, mais des suggestions qui seraient sans doute susceptibles d'être contrôlées en procédant à des expériences systématiques.

On pourrait notamment chercher à rendre moins ambiguës les conditions expérimentales de façon à ce qu'elles deviennent coercitives et imposent aux observateurs les impressions de la catégorie B. Ce résultat serait atteint sans doute si l'on augmentait considérablement la surface du fond et si l'on multipliait le nombre des objets qui se détachent sur celui-ci; ou mieux encore, si l'on remplaçait l'instantanéité physique de l'éclairement par une augmentation progressive de l'intensité lumineuse à partir de zéro. Ceci aurait vraisemblablement pour effet

d'accentuer la ségrégation de l'objet et de l'illumination, et permettrait d'étudier de plus près les conditions déterminantes de la permanence d'antériorité de l'objet [10]).

Enfin, les résultats des observations de la catégorie B montrent clairement nous semble-t-il, que la différence signalée plus haut entre le cas de l'illumination subite des objets et celui des projections lumineuses repose sur une confusion.

De fait, ce qui se passe lors des projections est tout à fait semblable en principe, à ce que l'on observe lors de l'éclairement des objets. La seule différence réside dans ce que l'on considère respectivement comme objets et comme éclairement; et c'est cela qui peut prêter à malentendu.

La projection en effet est, elle-même „lumière" (projections lumineuses!) et à supposer que la salle dans laquelle se trouve l'observateur soit absolument obscure, celle-ci se trouve „éclairée", ainsi que les objets qui s'y trouvent: écran, encadrement, spectateurs, etc. au moment où paraît l'image, exactement comme si l'on avait allumé soudain une lampe électrique. Il y a donc permanence de la salle et de son contenu et „création" apparente de la lumière [11]).

La confusion provient ici du fait que c'est une image représentant elle-même des objets qui remplit la fonction de la „lumière" et c'est à ce dernier titre qu'elle semble „créée" dans ce cas [12]); mais il n'en va pas nécessairement de même pour les

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[10]) Peut-être des expériences de ce genre pourraient-elles aussi provoquer l'impression d'une création simultanée de l'objet et de l'éclairement, au cas où la progression serait suffisamment lente pour qu'il y ait au début un stade non structuré et incolore, évoluant vers une organisation et une coloration définies.

[11]) Ce serait une erreur, il n'est peut être pas inutile d'y insister, de croire que l'apparition d'un „éclairement" porte toujours le caractère de „création". L'éclairement, aussi bien que les couleurs films ou que les couleurs d'objets peut, lui-aussi, sembler préexistant. C'est ce qui se réalise par exemple lorsque l'on ouvre brusquement la porte d'une chambre et que celle-ci apparaît d'emblée plus ou moins fortement éclairée.

[12]) Il n'est pas impossible que l'on puisse réaliser des conditions expérimentales telles, qu'une projection donne l'impression d'un simple éclairement d'une image peinte ou dessinée préalablement sur l'écran, mais ceci démontrerait uniquement que certains artifices opératoires permettent de provoquer une structure perceptive semblable à celle que nous avons observée à propos de l'expérience 3 et n'aurait aucune portée théorique spéciale.

objets représentés par l'image. Et il est intéressant de remarquer à ce propos que l'on retrouve, à l'intérieur de l'image, la dissociation entre les couleurs propres des objets et leur éclairement, et que cela peut justifier la permanence apparente des objets représentés. Que l'on songe seulement aux effets d'éclairement que l'on observe au cinéma lorsqu'on y voit allumer les lumières ou ouvrir les rideaux d'une chambre! C'est là un très bel exemple de la ségrégation des espaces et de la séparation des „systèmes", qui isole le champ propre de l'image, de celui de la salle dans laquelle elle est projetée [13]).

Passons maintenant à l'examen d'un autre cas de permanence, appartenant cette fois au domaine du tact.

Lorsque nous touchons un objet, ou que dans l'obscurité nous heurtons du pied un obstacle que nous n'avons pas vu, ou encore quand un objet, une balle par exemple, vient nous heurter dans le dos, nous n'avons point l'impression d'une „création" de ces objets au moment ou nous les sentons. Et il en va de même chaque fois que nous faisons quelques pas dans une chambre sans lumière, ou les yeux fermés; le sol dont nous percevons la résistance sous les pieds paraît également *préexister* à notre expérience actuelle.

On dira peut-être que ceci provient de ce que le contact brusque d'un objet nous donne immédiatement l'impression d'un corps physique solide et résistant, et que ce caractère serait déterminant de la permanence apparente.

Il ne manque pas d'arguments, d'ailleurs, qui paraissent plaider dans ce sens, et notamment la constatation que d'autres catégories d'impressions tactiles présentent l'aspect „création" lors de leur apparition, tels le chatouillement, la démangeaison, une douleur interne ou cutanée provoquée par une légère piqûre, ou encore de faibles sensations de contact, voisines du seuil. Cela étant, on pourrait imaginer que la permanence serait liée à l'objectivation, et l'absence de permanence à la subjectivation des réponses aux excitations tactiles-kinesthésiques.

Seulement notre discussion des exemples visuels est de nature à inciter à la prudence et à un certain scepticisme quant au rôle d'aspects de l'expérience tels que l'objectivation et la subjectivation.

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[13]) Cf. Michotte, loc. cit. p. 10.

Et au surplus, il est sage de se demander s'il est vraiment nécessaire de recourir à différents principes d'explication de la permanence ou de la non-permanence, principes qui varieraient suivant le domaine sensoriel que l'on considère.

Or, il suffit d'examiner la question d'un peu près pour se rendre compte que la distinction entre les aspects objectif et subjectif de l'expérience tactile intervient effectivement en l'espèce, mais d'une manière assez différente du rôle qu'on serait tenté de lui attribuer à première vue.

Il est clair que dans la perception du contact d'un objet extérieur avec notre peau, ou de la pression qu'il exerce sur elle, il ne s'agit nullement d'une *objectivation pure*, exclusive, car s'il en était ainsi nous aurions simplement l'impression d'un objet situé à distance, en tel endroit de l'espace tri-dimensionnel, comme dans le cas de l'expérience visuelle. Or, l'impression est tout autre; c'est celle *d'un objet en contact avec telle ou telle partie de notre corps*. L'excitation produite sur la surface cutanée donne donc naissance à une double impression, l'une „objective" qui est celle de la chose, l'autre „subjective" qui est celle du contact avec la peau. En d'autres mots, on se trouve en présence d'un cas de dédoublement phénoménal, le même système d'excitations produisant à la fois deux impressions différentes. Et il est bien évident que, si l'objet ne paraît pas naître au moment où il est senti, l'impression de contact par contre semble éphémère, elle est soudainement créée et s'évanouit ensuite.

L'impression du contact cutané joue donc en somme dans l'éxpérience tactile un rôle analogue à celui de l'éclairement dans l'expérience visuelle de la catégorie B. Et ceci est d'autant plus aisément compréhensible que le contact de l'objet avec la peau produit une déformation progressive de celle-ci et que par conséquent, les conditions objectives sont de nature à favoriser l'aspect évolutif de l'impression de contact, et ce qui s'ensuit: structure temporelle intrinsèque, limites propres, création et annihilation.

L'analyse des quelques cas discutés dans ces pages fournit semble-t-il une documentation suffisante pour tenter un essai de synthèse générale des problèmes concernant la permanence et la non-permanence phénoménales.

Il convient d'insister tout d'abord sur cette vérité que la

question de la permanence doit être envisagée dans la perspective de la *psychologie du temps*, à laquelle elle ressortit d'ailleurs par définition. Et, à ce point de vue, nous avons été amenés à souligner la distinction fondamentale entre le temps phénoménal externe aux processus et leur temps intrinsèque.

Tous les événements constitutifs de la vie psychique se déroulent évidemment dans le cadre du temps phénoménal dans lequel ils sont ordonnés les uns par rapport aux autres. Et il va de soi que le moment de l'apparition ou de la disparition d'un objet ainsi que sa durée ont toujours leur place marquée dans ce courant chronologique. Mais cela paraît d'importance secondaire pour ce qui concerne la permanence phénoménale.

On a vu, en effet que dans les cas où un objet apparaît ou disparaît brusquement sans subir aucune évolution interne, les impressions de naissance ou d'anéantissement, de préexistence, ou de survie font défaut. L'apparition marque le début de la *„présence"* de l'objet, mais les données phénoménales sont muettes au sujet du début de son *„existence"*. Et, lorsque l'objet demeure stable pendant toute la durée de sa perception, sa présence continue implique il est vrai son existence, mais celle-ci n'est assurée que pour cette tranche du temps et rien n'indique sa pérennité, en deçà ou au delà de la période de présence.

Or tout ceci ne touche guère les véritables problèmes de la permanence, car ceux-ci visent „l'existence" même de l'objet, son origine ou son terme et sa continuité dans le temps: préexistence et survie, maintien de son identité à travers les modifications diverses qu'il peut subir.

Le résultat de notre enquête a montré que ces aspects de la permanence et de la non-permanence pouvaient se manifester comme données spécifiques d'expérience, sous la forme de phénomènes caractéristiques répondant à des conditions d'excitation bien déterminées, et que, au surplus, ils étaient intimement liés au fait que les processus possèdent une structure temporelle intrinsèque qui constitue leur temps propre.

Pour ce qui est des impressions de création et d'anéantissement d'un objet, elles correspondent au fait que l'objet même semble dans certaines conditions se former, se développer au moment de son apparition ou au contraire se replier sur lui-même quand il disparaît. Il „devient", ou il „s'évanouit" progressivement, et cette évolution est un processus intrinsèque

qui n'est pas simplement incorporé dans le temps „externe", mais qui a son rythme à lui et possède par conséquent des limites propres, lesquelles marquent nécessairement le début ou le terme de „l'existence" de l'objet.

Quant à la permanence proprement dite, nous en avons mentionné trois cas qui sont assez représentatifs sans doute de la grande majorité de ceux que l'on rencontre dans la vie courante: le Changement, l'effet Ecran et l'effet Illumination. Et, au point où nous sommes arrivés à présent, il est tout indiqué de les comparer et de tenter de les rapprocher les uns des autres.

L'impression de changement se caractérise par le maintien de l'identité de l'objet malgré les modifications apparentes de certaines de ses propriétés, et nous avons interprété ce phénomène en faisant appel à la notion de Gestalt, de „cachet spécifique global" correspondant à l'union et à la coordination de l'ensemble des propriétés constitutives de l'objet.

Cela étant, la possibilité de la permanence de l'objet est manifestement liée au fait que le cachet spécifique dont il est question est une donnée phénoménale sui generis, jouissant d'une autonomie relative par rapport aux diverses propriétés particulières qu'il englobe.

D'autre part, un instant de réflexion permet de reconnaître que ce cas, classé jusqu'à présent parmi ceux de permanence de continuité, n'est au fond qu'un cas de permanence d'antériorité. En effet, le changement lui-même est un processus qui, si rapide soit-il, apparaît toujours comme une *évolution* partielle de l'objet; il se présente donc comme un „devenir" et possède de ce chef une limite temporelle antérieure propre. Mais cette évolution se bornant à certaines propriétés et n'atteignant pas le cachet spécifique lui-même, sa limite antérieure n'est pas celle de l'existence de l'objet; et celui-ci *préexiste* au changement qu'il subit. Lorsqu'on parle de permanence de continuité dans ce cas, il s'agit simplement d'une autre perspective qui fait mettre l'accent sur le fait que l'objet déjà existant, *continue à exister* ultérieurement. Et l'on voit ainsi que (exception faite pour le cas de la continuité absolue qui ne pose en réalité guère de problèmes) la distinction entre permanence de continuité et permanence d'antériorité ou de postériorité, ne repose en somme

que sur la différence des points de vue que l'on adopte pour décrire ou pour exprimer une même donnée phénoménale.

Dans le cas de l'illumination, la préexistence d'antériorité de l'objet se présente dans des conditions très différentes, en ce sens que l'objet n'est pas visible et n'existe même pas phénoménalement avant son éclairement. Et elle se vérifie néanmoins, croyons-nous, parce que l'illumination possède une limite temporelle antérieure propre, en deçà de laquelle l'objet paraît s'étendre dans le passé (un passé indéfini par nature), à l'instant où on le perçoit, ce qui se traduit par une impression spécifique de „déjà là".

L'effet Ecran enfin, a, lui aussi, ses caractères particuliers, étant donné que les parties, encore (ou déjà) cachées de l'objet, existent phénoménalement par suite de l'appartenance exclusive à l'écran, de la frontière qui sépare celui-ci de l'objet recouvert. Et ceci se produit bien que ces parties ne soient pas actuellement visibles et qu'elles restent nécessairement indéterminées dans leur grandeur.

Pour ce qui est du temps, l'apparition de l'objet se fait également d'une manière spéciale puisque ce dernier semble sortir de derrière l'écran, glisser progressivement. Il y a donc encore une fois évolution temporelle, et celle-ci a sa limite antérieure propre qui marque le commencement de l'apparition de l'objet. Il ne s'agit cependant pas d'une „création" de celui-ci parce que précisément, les conditions spatiales assurent sa préexistence, et sa rigidité apparente [14]). L'objet lui-même n'est donc le siège d'aucune évolution interne, il paraît d'emblée parfaitement stabilisé comme dans le cas de l'illumination. Et, par conséquent, la limite temporelle n'appartient qu'au mouvement de glissement et ne marque que le début de l'apparition de l'objet, sans l'affecter lui-même.

La comparaison de ces trois cas est extrêmement instructive. Elle montre en effet que, malgré leur diversité, ils répondent tous à un schéma d'organisation structurale de la perception, qui est semblable en principe.

Dans les trois exemples cités, il se produit un dédoublement

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[14]) Voir à ce propos: Knops. loc. cit. pp. 589 seq.

phénoménal, puisque le système des excitations aboutit à donner deux catégories distinctes d'impressions jouissant d'une autonomie relative les unes par rapport aux autres, mais intimement liées: le cachet global spécifique et les propriétés particulières de l'objet, pour le changement; l'éclairement et l'objet, pour l'illumination; l'écran et l'objet recouvert, pour l'effet Ecran.

De plus, chaque fois, l'un de ces aspects de l'expérience globale subit une évolution progressive et possède de ce chef une limite temporelle antérieure propre; ce sont la modification apparente des propriétés, pour le changement; l'expansion de la lumière, pour l'éclairement; et le glissement de l'objet, pour l'effet Ecran. Quant à l'autre aspect de l'expérience, il est au contraire stabilisé: cachet spécifique global de l'objet dans le cas du changement, objet éclairé dans celui de l'illumination, objet découvert dans celui de l'effet Ecran.

Il y a donc un parallélisme évident dans les structures perceptives, et ceci donne la clé de toute la question, car il n'est pas difficile de se rendre compte que ce genre de structure est le seul qui puisse correspondre à une permanence d'antériorité; et que de plus, il *doit* nécessairement en présenter l'apparence. En effet, la permanence d'antériorité suppose par nature *un décalage des limites temporelles*, étant donné qu'il ne peut y avoir, par définition, de préexistence que si l'objet existe déjà avant qu'un événement qui le concerne commence à se produire. Sa limite temporelle à lui (pour autant qu'il en ait une au point de vue phénoménal) ne peut donc coïncider avec celle de l'événement nouveau. Or ceci n'est possible que s'il y a dédoublement phénoménal, c'est à dire si l'expérience totale présente deux aspects distincts fortement unis, dont l'un possède une frontière temporelle qui ne coïncide pas avec le début de l'existence de l'autre.

Le dédoublement phénoménal apparaît ainsi comme une condition fondamentale de la permanence d'antériorité, et pour le reste, le problème se ramène en dernière analyse à celui de la coïncidence ou de la non-coïncidence des limites temporelles. Bref, la permanence des objets est non seulement, dans certaines conditions, une donnée phénoménale originelle, mais de plus, elle se comprend aisément à partir des principes qui gouvernent l'organisation structurale des perceptions.

Les connaissances acquises au cours de la vie élargissent sans

doute considérablement la portée et la signification de la permanence, en la liant au concept de substance, et en confirmant apparemment nos convictions spontanées à son égard. Mais on trouve déjà sur le plan perceptif même, ici comme en d'autres matières, celles de la causalité et de la réalité notamment, une véritable préfiguration phénoménale des résultats de l'élaboration des données de l'expérience par la pensée non-critique (au sens philosophique du mot), en fonction de laquelle se développe normalement le comportement humain.

UNE EXPERIENCE SUR LA PSYCHOLOGIE DU HASARD CHEZ L'ENFANT: LE TIRAGE AU SORT DES COUPLES

par

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On pose sur la table une collection nombreuse d'éléments A (par exemple 15 jetons jaunes), un peu moins de B (p. ex. 10 jetons rouges), encore moins de C (p. ex. 7 jetons verts) et très peu de D (p. ex. 3 jetons bleus). On laisse sur la table des témoins de même couleur et de mêmes quantités permettant à l'enfant de se rappeler ces inégalités sans effort, et l'on met les 4 collections dans un sac que l'on secoue jusqu'à mélange complet. L'enfant plonge alors la main dans le sac et la consigne est d'en retirer un certain nombre de couples successifs: on lui demande alors, avant chaque nouveau tirage, de prévoir le couple le plus probable (les éléments déjà sortis sont naturellement arrangés de façon bien visible sur la table, pour que l'enfant puisse juger de ce qui reste dans le sac, mais on ne lui donne pas d'explication à cet égard). On peut aussi, avec les plus petits, faire prévoir la sortie d'un seul jeton à la fois.

Au cours du stade I, l'enfant ne cherche pas à prévoir en fonction du nombre des combinaisons possibles, mais selon toutes sortes de critères, parmi lesquels intervient parfois, mais à titre de simple cas particulier, la majorité des éléments en jeu. Au cours du stade II, il y a recherche de rapports quantitatifs, mais sans un calcul réadapté, au fur et à mesure des tirages, au nombre des éléments qui restent à l'intérieur du sac (par opposition aux jetons déjà sortis). Au cours du stade III, enfin, la probabilité est quantifiée en fonction des données simples indiquées à l'instant.

1. LE STADE I: ABSENCE DE PROBABILITE SYSTEMATIQUE. — Voici d'emblée quelques exemples, portant sur le tirage d'éléments isolés:

BER (5; 1) 6 rouges, 2 bleus: „Si tu prends un jeton dans le sac,

n'importe lequel, mais sans regarder dedans, de quelle couleur tu crois qu'il sera plutôt? — „*Rouge.* — Pourquoi? — *Parce que j'aime bien le rouge.* — (6 bleus et 2 rouges). Même question. — *Rouge* — Mais il y a beaucoup de bleus. Tu ne crois pas que ce sera plutôt un bleu? — *Oui.* — (6 bleus et 1 blanc) [1]) — *Blanc.* — Pourquoi? — *Parce que le blanc est le premier* (il montre l'échelle des témoins sur la table). — Mais on a bien mélangé dans le sac, tu vois comme ça (on mélange sur la table). Maintenant le blanc n'est plus le premier. Prends en un dans le sac. De quelle couleur tu crois qu'il va être? — *Blanc.* — Pourquoi? *Parce que vous avez mélangé*".

(6 blancs, 1 rouge, avec comme chaque fois, les témoins correspondants sur la table). — *Rouge.* — Pourquoi? — *J'aime le rouge.* — Regarde ce que tu as pris. — *Blanc.* — Pourquoi? — *Parce qu'il était au coin du sac*".

Leu (5; 8) 6 rouges, 4 bleus, 1 blanc. Les témoins sont sur la table et Leu secoue lui-même le sac: „Si tu prends une puce dans le sac, n'importe laquelle, de quelle couleur elle sera plutôt? — *La blanche.* — Pourquoi? — *Parce qu'il n'y a qu'une blanche.* — (On enlève la blanche dans le sac et sur la table). — Maintenant, si tu en prends une n'importe comment, de quelle couleur elle sera plutôt? — *Bleue.* — Pourquoi? — (Il ouvre sa main) *Rouge*! Pourquoi c'est une rouge et pas une bleue? Il y a une raison? — *Non* — (6 rouges 1 blanc). — *Rouge.* — Pourquoi? — .... — (4 blancs, 4 rouges) Qu'est-ce qui sortira? — *Rouge.* — Pourquoi? — .... — C'est plus sûr que c'est une rouge? — *Oui.* — Mais pourquoi? ...."

Mon (5; 10) 6 bleus, 3 rouges: „*Rouge.* — Pourquoi? — *Parce que.* — (6 bleus, 2 rouges). — *Bleu.* — Pourquoi? — *Parce que.* — (3 bleus, 6 rouges). — *Bleu.* — Pourquoi? — .... — (12 rouges et 1 blanc). — *Blanc.* — Pourquoi? — *Parce qu'il y a un blanc.* — (12 blancs et 1 rouge). — *Rouge.* — Pourquoi? — *Parce qu'on en a mis un*".

Scha (6; 3) 1 rose, 2 jaunes, 5 rouges, : „*Rouge.* — Pourquoi? — *Parce qu'on en a beaucoup.* — (On recommence en ajoutant 6 bleus) Et maintenant? — *Un jaune.* — Pourquoi plutôt un jaune? — *Je ne sais pas.* — (1 rose, 2 jaunes, 2 rouges, 6 bleus). — *Je*

[1]) On répète naturellement chaque fois la question entière, en variant les mots.

crois que c'est le rose qui sortira. — Pourquoi? — .... — (4 bleus et 4 jaunes). On peut savoir ou pas? — *Plutôt un jaune.* — Pourquoi? — *C'est plus joli*" —

1 bleu, 2 jaunes, 3 verts, 4 rouges et 5 roses: „Si tu en prends deux, n'importe comment, de quelle couleur tu penses qu'ils seront? — *Jaunes.* — Pourquoi? — *Parce qu'il y en a deux.* — Et maintenant (4 jaunes, 4 rouges, 4 roses)? — *Rouge et rose.* — Pourquoi? — *parce que c'est joli*".

Lan (6; 6) 2 bleus et 6 rouges: „*Rouge.* — Pourquoi? — *Parce qu'il y en a plus.* — (2 blancs, 5 rouges et 5 bleus). — *Blanc.* — Pourquoi? — *Parce qu'il n'y en a pas beaucoup et qu'il y a la même chose de rouges et de bleus.* — (1 bleu, 3 rouges et 6 blancs). — *Blanc.* — Pourquoi? — *Parce qu'il y en a plus.* — (3 roses, 3 rouges, 2 blancs). — *Rose.* — Pourquoi? — *Parce qu'il y a 3 roses et 3 rouges.* — (idem + 4 verts) Et maintenant? — *Rouge, parce qu'il n'y en a pas beaucoup*".

Fin (6; 4) 2 blancs et 8 bleus: „*Un bleu, parce qu'il y en a plus.* — (4 blancs et 4 rouges). — *On ne peut pas savoir parce qu'il y en a autant de blancs et de rouges.* — (1 bleu, 2 verts, 2 roses, 3 rouges) *Un rouge parce qu'il y en a le plus.* — Et des verts et des roses, qu'est-ce qui viendra plutôt? — *Une rose, parce que c'est à côté du rouge* (comme teinte ou sur l'échelle posée comme aide-mémoire).

1 bleu, 2 verts, 3 roses, 4 rouges, 5 bleus, 6 jaunes: „Si tu en prends deux, qu'est-ce qui viendra plutôt? — *Deux jaunes, parce qu'il y en a le plus* (il y en a d'autres 15 autres!). — Et si ce ne sont pas les jaunes? — *Deux bleus.* — Et sans ça? *Deux rouges.* — Si on tire au hasard, ça peut être de couleurs différentes? — *Alors une rose et une rouge* (en tant que couleurs voisines)".

Per (7; 3) 1 blanc, 2 roses, 3 verts, 4 rouges, 5 bleus et 6 jaunes: „Si tu en tires un au hasard, n'importe comment, lequel ce sera plutôt? — *Le blanc.* — Pourquoi? — *Parce que c'est le premier.* — Mais si je mélange bien, c'est toujours le premier? — *Oui.* — (On enlève le tout et on secoue à nouveau). — *Un rose.* — (6 bleus et 3 blancs). — *Un bleu.* — Pourquoi? — *Parce qu'il y en a beaucoup*".

6 blancs, 5 jaunes, 4 bleus, 3 verts, 2 rouges et 1 rose: „Si tu tires deux jetons à la fois, qu'est-ce qui viendra plutôt? — *Rouges.* — Pourquoi? — *Parce qu'il y en a deux.* — Et si on

mélange bien? — *Blanc et rouge.* — Et sans ça? — *Rouge et jaune.* — Et si c'est tout à fait bien mélangé? — *Alors blanc et rose* (= les deux extrêmes sur l'échelle).

Telles sont les réactions courantes de ce premier stade. Il est un trait général qui frappe avant tous les autres et qui confirme avec une singulière netteté ce que nous avons sans cesse constaté au même niveau, tout se passe comme si les jeunes sujets ne tenaient pas compte du brassage des jetons dans le sac, autrement dit ne comprenaient pas la nature du mélange en tant que système d'interférences fortuites. C'est ainsi que pour servir d'aide-mémoire à l'enfant nous posons sur la table autant de jetons que dans le sac, en les disposant en escalier selon leurs couleurs: 1 blanc, 2 verts, 3 roses, etc., pour que l'enfant perçoive les différences sans avoir besoin de compter. Or, il s'est trouvé que beaucoup de sujets, tout en sachant fort bien que les jetons du sac étaient mêlés (non seulement on le répète sans cesse, mais ce sont les enfants eux-mêmes qui secouent le sac pour brasser) ont raisonné comme si l'ordre de l'échelle modèle allait régler le résultat des tirages eux-mêmes, autrement dit comme si les jetons, quoique mêlés dans le sac, obéissaient toujours aux rapports déterminés par les rangs initiaux. P. ex. Ber commence par prévoir que sur 6 jetons bleus et 1 blanc, c'est celui-ci qui sortira plutôt lors du tirage d'un seul élément: la raison donnée est qu'il est „le premier" (sur l'échelle!) sans que le sujet s'occupe des quantités en présence; lorsqu'on insiste sur le brassage en indiquant qu'ainsi le blanc n'est plus le premier, Ber annonce à nouveau le blanc, mais cette fois „parce que vous avez mélangé", comme si son rang initial ne se perdait pas mais permettait au contraire de le repérer au sein des éléments brassés. Le premier des critères auquel obéit le choix de l'enfant est donc le rang initial des données.

Un second critère, également fréquent, revient au même quant à l'incompréhension du brassage et au mépris de la quantification possible. Lorsqu'on annonce au sujet qu'il va tirer un seul jeton, au hasard, ou deux jetons quelconques, il lui arrive souvent de prévoir que la couleur sortante sera précisément celle qui est représentée le même nombre de fois sur l'échelle modèle, c'est-à-dire celle de l'élément unique ou de deux seuls éléments. Autrement dit, il ne juge nullement que la couleur la plus abondamment représentée a plus de chances de sortir au tirage,

mais au contraire que le fait de chercher un seul ou deux seuls jetons déclenchera mystérieusement l'apparition de l'élément ou du couple seuls de leur espèce. C'est ainsi que LEU, sur une collection de six puces rouges, quatre bleues et une blanche, prévoit qu'il va tirer la blanche, „parce qu'il n'y a qu'une blanche" MON, sur un blanc et douze rouges prédit la sortie du jeton blanc „parce qu'il y a un blanc" et sur douze blancs et un rouge prédit le rouge „parce qu'on en a mis un". SCHA, de même, prié de sortir un couple au hasard, annonce deux jaunes (sur 15 éléments) „parce qu'il y en a deux" seuls (dans l'échelle modèle). PER également, sur 21 éléments, croit qu'il va tirer un couple de rouges „parce qu'il y en a deux". C'est sans doute à ce même argument qu'il faut rapporter la réponse étrange, parfois rencontrée, qui motive le choix de la prévision par le petit nombre des éléments représentés. Ainsi, LAU, après avoir répondu en apparence correctement, pour deux bleus et six rouges „rouge, parce qu'il y en a plus", déclare, pour deux blancs, cinq rouges et cinq bleus, „blanc, parce qu'il n'y en a pas beaucoup, et qu'il y a la même chose de rouges et de bleus": du moment que l'on ne désire qu'un jeton, veut sans doute dire l'enfant, il ne peut sortir des deux collections les plus nombreuses, surtout qu'elles sont égales, aucun motif ne pouvant décider en faveur de son choix individuel, tandis que, sur deux blancs, il n'y a qu'à se décider pour l'un des deux.

Un troisième type de critère tient aux qualités intrinsèques des éléments à prévoir, et cela toujours au mépris des effets du brassage et des quantités en jeu. C'est ainsi que FIN, qui pourtant invoque au début le nombre des éléments, pense que si deux jetons doivent sortir sans être de la même couleur, ils seront „rouge et rose" parce que ce sont deux couleurs voisines. De même SCHA annonce „rouge et rose, parce que c'est joli", c'est-à-dire que ces couleurs s'accordent bien entre elles.

Un quatrième critère est la préférence subjective, comme si le hasard était équivalent à l'arbitraire simple: „l, rouge", prévoit ainsi BER „parce que j'aime bien le rouge" et SCHA „plutôt le jaune, c'est plus joli".

Enfin le cinquième critère paraît dénoter un sens plus précis de la probabilité: c'est la quantité elle-même, de deux collections, la plus nombreuse a plus de chances de donner lieu à un élément sortant, semble dire l'enfant. Mais il faut se garder d'une inter-

prétation aussi proche de notre vision des choses, précisément du fait que ce dernier critère est sans cesse mêlé aux autres. Pour être précis, il faut distinguer deux cas, de significations bien différentes, avec tous les intermédiaires entre deux. Dans le premier, qui est celui des plus jeunes sujets, la quantité n'est aux yeux de l'enfant, qu'une qualité comme les autres: de même que le jeton peut sortir parce qu'il est le premier, ou qu'il est représenté par un seul individu sur l'échelle, ou qu'il est joli, etc., de même il peut sortir parce qu'il y en a beaucoup de son espèce, sans que cela résulte d'une compréhension des effets du brassage. Ainsi SCHA, pour un rose, deux jaunes et cinq rouges prévoit „rouge, parce qu'on en a beaucoup" (à noter ce „on" qui signifie „nous" dans le parler populaire romand et non pas „il y en a"), mais, lorsqu'on ajoute six bleus, il prévoit „jaune" sans raison et passe ensuite aux critères subjectifs. Le second cas est, au contraire, celui des sujets qui semblent s'acheminer vers la quantification effective, en vertu d'une intuition des effets du brassage, mais qui n'en renoncent pas moins aux autres critères en cas de besoin. C'est ainsi que FIN commence par des réponses qui annoncent celles du stade II: „un bleu parce qu'il y en a beaucoup", etc... mais pour un bleu, deux verts, deux roses et trois rouges, il juge en probabilité de seconde zone „un rose, parce que c'est à côté du rouge", ce qui est un retour au critère du rang ou des affinités électives.

Or, on constate que ce second cas augmente de fréquence vers la fin du stade, ce qui montre bien qu'il s'agit d'une intuition naissante de la probabilité, qui va se développer au cours du stade II. On peut donc, à son sujet, dinstinguer comme nous l'avons fait parfois, un niveau I B intermédiaire entre le niveau élémentaire I A et le niveau II lui-même.

Deux conclusions découlent de cette description. La première est que, comme nous avons pu le constater déjà à plusieurs reprises, l'idée du mélange, qui est au point de départ de celle du hasard, est loin d'être aussi primitive et aussi simple à saisir qu'il pourrait le sembler. Qui dit mélange dit, en effet, indépendance de plus en plus complète des éléments rendus contigüs par leurs trajets interférents (c'est bien pourquoi le mélange est le prototype de cette interférence entre séries indépendantes au moyen de laquelle Cournot définissait le hasard). Or, les sujets de ce stade I raisonnent, au contraire, comme si les éléments une

fois mêlés conservaient les qualités qu'ils tenaient de leurs arrangements antérieurs, précisément détruits par le mélange: en particulier les billes sont censées maintenir, dans le sac où elles sont brassées, les mêmes relations entre elles que sur l'échelle-témoin servant d'aide-mémoire à l'enfant. Ces réactions curieuses rappellent une observation que nous avions faite jadis avec A. Szeminska: pour certains jeunes sujets, une collection A paraît plus grande qu'une collection B lorsque cette collection A est tirée d'un ensemble A′ plus grand que l'ensemble B′ dont est extrait B (p. ex. 6 jetons tirés d'un tas de 30 feront „plus" que 6 autres jetons extraits d'un tas de 10!). En d'autres termes, le mélange tel qu'il est conçu à ce niveau antérieur à 7—8 ans, n'est, pour ainsi dire, pas encore irréversible! La chose est d'autant plus paradoxale que, comme nous l'avons déjà remarqué à propos du stade I du chap. I (au cours duquel les sujets attribuent aux perles brassées par un mouvement de bascule le pouvoir de retrouver leur place initiale), l'enfant de ce niveau n'est capable ni de conservation rationnelle ni de réversibilité opératoire. Avant le stade II où s'élaborent corrélativement les opérations réversibles et la notion du mélange irréversible, ce qui est donné n'est qu'un monde d'intuitions perceptives et subjectives (phénoménisme × égocentrisme), dont les qualités se conservent par simples transferts ou persévération, le contraire de cette pseudo-conservation intuitive étant sans plus l'arbitraire et non pas encore le hasard.

Or, si la notion du hasard dérive de l'intuition du mélange, le jugement de probabilité consistera à anticiper les possibilités de retrouver les éléments mélangés, une telle anticipation ne pouvant alors être fondée que sur les rapports quantitatifs en jeu. La seconde conclusion à tirer des faits précédents est alors que d'une telle notion du mélange irréversible, l'enfant de ce niveau ne parviendra point à l'idée de structurer ses évaluations probabilistes en les fondant sur une quantification systématique. Bien plus, il n'en aura même pas le pouvoir puisque cette quantification supposerait un système d'opérations à la fois numériques et combinatoires dont l'absence explique précisément pourquoi l'élaboration du concept de mélange demeure inachevé.

Lorsque Mon, p. ex., en présence de 12 jetons blancs et un rouge, brassés dans un sac, se fait fort de tomber directement sur le rouge parce qu'il est unique, il témoigne non seulement

d'une faible intuition du mélange, mais encore d'une notion bien rudimentaire de la probabilité, dans le sens des opérations nécessaires pour retrouver au sein de la collection mélangée telle ou telle catégories d'éléments. En effet, dans le cas particulier le jugement de probabilité consiste, étant donnée une main qui circule dans le sac convenablement brassé, à anticiper les rencontres entre cette main et les éléments désirés. On voit alors en quoi cette anticipation conduit nécessairement à des opérations de quantification. Pour retrouver le jeton rouge à coup sûr, il suffirait de mettre la main sur la totalité de la collection, c'est-à-dire de retirer 13 éléments sur les 13 présents: il y aurait alors certitude ou probabilité $13/13 = 1$. Pour retrouver telle catégorie d'éléments, il s'agit par contre de la considérer comme une fraction du tout, c'est-à-dire comme une partie à comparer quantitativement à la partie complémentaire, ce qui revient à débiter la certitude (valeur 1) en fractions correspondantes aux parties désirées. Or, en s'attendant à une sortie plus probable du jeton rouge, Mon raisonne comme si sa main n'était pas exposée à des rencontres 12 fois plus nombreuses avec les jetons blancs, c'est-à-dire comme si le jeton rouge unique n'était pas simplement le treizième d'un tout à considérer en son ensemble. Il attribue donc une sorte de correspondance directe entre sa main et le jeton rouge considéré absolument et en son unicité, au lieu de fractionner la certitude 13/13 en deux probabilités 1/13 et 12/13, ce qui donnerait tout l'avantage aux jetons blancs. En d'autres termes il n'a encore aucune notion de probabilité, tandis que les sujets du niveau I B, qui font appel à la quantité caractérisant la catégorie de l'élément désigné s'orientent vers la vraie probabilité précisément parce qu'ils répartissent l'ensemble total en catégories soutenant entre elles des relations quantitatives, au lieu de raisonner sur tel élément ou telle qualité isolés.

Psychologiquement comme logiquement, le jugement de probabilité est donc solidaire de l'élaboration d'opérations quantitatives de combinaisons: tandis que le hasard résulte d'un mélange irréversible, le jugement de probabilité consiste au contraire à réduire par la pensée ce brassage à un système de combinaisons tel que l'on puisse retrouver les éléments mélangés en se fondant sur leurs rapports quantifiés de parties à parties et de fraction.

2. Le Stade II: Debuts de probabilite quantifiee. — Cette quantification de la probabilité débute dès le stade II sitôt constituée l'idée du mélange proprement dit. Mais les sujets de ce niveau II, au lieu de tenir compte, après chaque tirage, du nombre des éléments de chaque catégorie qui subsistent dans le mélange, oublient de dénombrer les jetons déjà sortis du sac et raisonnent ainsi par catégories fixes:

Fre (8; 6) 8 jaunes, 4 rouges, 2 verts, 1 bleu: „Si tu en sors deux à la fois, sans regarder, qu'est-ce qui a le plus de chance de sortir? — *Deux jaunes* — Pourquoi? — *Parce qu'il y en a beaucoup* (il en sort deux). — Et maintenant si tu en sors un? — *Encore un jaune* (il sort un rouge). — Et maintenant? — *Un rouge* (il sort le bleu). — Et maintenant? — *Un vert.* — Et après? *Un jaune.* — Et après? *Un rouge.* — Et ensuite? — *Un vert*". Il suit donc l'ordre des couleurs par fréquences de croissances.

Pie (9; 3) 8 roses, 6 jaunes, 4 blancs et 1 vert: „Si tu en prends deux, etc.? — *Rose et vert* (les extrêmes). — Essaie (il sort rose et blanc). Pourquoi rose et blanc? — *Il y en a beaucoup.* — Et maintenant? — *Blanc et jaune.* — Et ensuite? — *Vert et blanc* (il descend le long de l'échelle)".

Dau (9; 6) 8 rouges, 5 jaunes, 2 roses et 1 vert: il annonce successivement les couples rouge-jaune, rouge-rose, rouge-vert, associant ainsi un élément de la collection la plus nombreuse à un élément de chacune des autres en ordre de fréquence décroissant.

Giu (10 ans) 15 verts, 12 jaunes, 8 rouges, 3 bleus: „Si tu en prends deux qui est-ce qui a le plus de chances de sortir? — *Deux verts ou un vert et un jaune.* (Il sort deux verts). — Et maintenant? — *Vert et jaune* (il sort jaune et bleu) — Et maintenant? — *Deux rouges.* — Regarde ce qui est sorti: on a plus de chances pour les verts ou les rouges? — *Rouges parce qu'on a déjà deux verts* (il en reste 13!) — Essaie (il sort deux verts). — Pourquoi as-tu de nouveau deux verts? — *Parce qu'il y avait plus de verts que d'autres.* — Et maintenant? — *Vert et rouge* (il sort vert et bleu). — Et maintenant? *Deux jaunes.* — Pourquoi? — *Parce qu'il n'y en a pas beaucoup de sortis.* — Et maintenant, un bleu? — *Je ne crois pas*".

Chev (10 ans) 8 jaunes, 6 rouges, 4 blancs, 2 verts: „*Jaune et rouge* (il sort deux rouges). — Et puis? *Deux jaunes* (il sort jaune et rouge) — Et puis? — *Deux verts.* — Pourquoi? — *On n'en a pas encore eu*".

Le sens de ces réactions est clair. D'une part, tous ces sujets tiennent compte de la quantité, ce qui montre bien qu'ils conçoivent le mélange comme effectif et comprennent que la main plongée dans le sac a d'autant plus de chances de rencontrer un élément appartenant à une certaine catégorie que celle-ci est plus nombreuse. Seulement si ces notions servent bien de départ, elles ne valent plus dans la suite: au lieu de faire le compte de ce qui reste dans le sac après chaque nouveau tirage et de reproduire le même raisonnement quantitatif à propos de ces résidus successifs, le sujet se borne à suivre un ordre de fréquences décroissantes en se fondant sur la distribution initiale ou même oublie les inégalités et raisonne par catégories comme si elles étaient équivalentes.

C'est ainsi que certains sujets, tout en partant de la distribution quantitative, passent encore d'un extrême à l'autre, comme Pie qui prévoit à titre de couple le plus probable un rose (8 éléments) et un vert (un élément) et Chev qui annonce au troisième tirage deux verts (deux éléments sur 20) parce que „on n'en a pas encore eu". Cette première méthode rappelle les procédés du premier stade, bien que s'alliant avec une préoccupation nette de quantité.

Une seconde méthode consiste à suivre l'ordre décroissant des fréquences, comme Fre qui institue même une sorte de tour de rotation, ou Dau qui associe pour chaque couple un élément de la catégorie la plus nombreuse avec un élément de chacune des autres en progression décroissante. Il va de soi que cette seconde méthode, tout en reposant sur un principe de quantification ne tient pas compte des relations exactes entre les fréquences.

Enfin la troisième méthode consiste à partir des catégories les plus nombreuses, mais ensuite à prévoir simplement en fonction des couleurs peu sorties sans tenir compte des quantités restantes. P. ex. Giu prévoit au troisième couple deux „rouges parce qu'on a déjà deux verts" (or il reste 13 verts et 8 rouges) et, plus tard, deux jaunes „parce qu'il y en a pas beaucoup de sortis". Ce troisième procédé, tout en négligeant la relativité des catégories successivement modifiées conduit néanmoins insensiblement à tenir compte des quantités restantes.

3. Le Stade III et Conclusions. — Les sujets du stade III parviennent à quantifier à nouveau les probabilités après chaque

nouveau tirage, montrant ainsi qu'ils conçoivent ainsi l'ensemble des rapports en jeu comme se modifiant lors de chaque transfert d'éléments:

Sto (10 ans) 12 blancs, 10 rouges, 5 verts et 3 bleus: „*Un blanc et un rouge* (il sort deux verts). — Et maintenant? — *Blanc et rouge* (il les sort effectivement et continue ainsi plusieurs fois puis dit:) *On a maintenant moins de chances qu'avant de sortir blanc et rouge parce qu'on en a beaucoup enlevés. Ce sera plutôt un vert et un bleu*".

12 roses et 6 rouges: „*Il y a plus de chances pour deux roses parce qu'il y en a deux fois plus* (il en sort deux). *Il y a toujours plus de chances pour les roses mais moins qu'avant* (il les sort). *Maintenant il y a presque la même chance pour les deux*".

Laur (12; 3) 14 verts, 10 rouges, 7 jaunes, 1 bleu: „*Deux verts* (il sort rouge et jaune). — Et maintenant ce sera quoi? — *Deux verts* (il les sort). — Et maintenant? — *Il y a encore plus de chances pour deux verts, puisqu'il en reste 12 et il n'y a plus que 9 rouges et 6 jaunes.* — Essaye. — (Il sort vert et bleu) *Tiens, voilà l'unique bleu qui sort.* — Et maintenant? — *C'est toujours deux verts qui ont plus de chances*".

Si simples que soient ces réponses, on voit que leur difficulté pour le sujet consiste à réajuster après chaque nouveau tirage l'ensemble des rapports en jeu (de même que pour découvrir la méthode systématique des combinaisons, la difficulté sera de coordonner les combinaisons de chaque couleur successivement avec toutes les autres, même engagées ailleurs). — Il s'agit donc bien de revenir sans cesse au point de départ au lieu de considérer les termes du problème comme statiques ou indépendants les uns des autres. Il se produit ici quelque chose de comparable à ce qui est la modification des systèmes de référence au fur et à mesure d'un mouvement qu'ils servent par ailleurs à repérer [1]) et c'est pourquoi l'intervention des opérations formelles semble nécessaire en ce cas.

La conclusion essentielle à tirer des observations précédentes est que les notions de hasard et de probabilité sont de nature

[1]) Voir p. ex. *Les notions de mouvement et de vitesse chez l'enfant*, Presses universitaires de France.

essentiellement combinatoire. C'est pourquoi nous avons étudié sur les mêmes sujets le dévoloppement des opérations concrètes de combinaison. Mais, sans connaître encore le détail de cette construction opératoire il est intéressant de chercher à discerner, dès les présents faits, l'intervention croissante de schèmes combinatoires dans les réactions spontanées des sujets.

Il est ainsi évident qu'au cours du stade I de tels schèmes ne se manifestent point encore; cela n'a d'ailleurs rien de surprenant puisqu'avant 7 ans en moyenne l'enfant n'est pas capable de sériations opératoires ou d'emboîtements de classes ni *à fortiori* d'une compréhension des opérations numériques élémentaires. Néanmoins il est clair que, dès ce niveau I le sujet possède certaines intuitions empiriques lui fournissant une anticipation de ce que sera plus tard la connaissance du hasard. Or, ces constatations empiriques pourraient déjà donner lieu à une interprétation combinatoire: mais l'enfant n'y parvient pas et c'est précisément parce qu'il manque une telle structuration opératoire qu'il échoue à construire la notion du hasard. C'est ainsi qu'il connaît l'existence du mélange, à titre de brassage matériel: mettant parfois ses jouets en ordre et les entassant d'autres fois pêle-mêle, il sait bien que de retrouver un objet particulier est plus facile dans la première situation que dans la seconde; et lorsque, dans cette dernière, il veut retrouver ce qu'il a perdu, il voit bien que sa réussite dépend du nombre des èlèments et des diverses associations possibles entre eux. Il a donc sans cesse l'occasion d'éprouver directement les coups mêmes du sort, heureux ou malheureux. Seulement, et c'est là l'enseignement des faits étudiés en cet article, l'intuition d'un brassage matériel n'est pas encore la compréhension du mélange combinatoire, et sans une structuration active, l'expérience ne suffit pas à conduire, à elle seule, à l'idée de hasard ni *à fortiori* à celle de probabilité.

Que pourrait, en effet, l'expérience pure en présence d'un brassage ou, ce qui est équivalent, d'un système de positions et de déplacements imprévisibles (et même, de façon plus générale encore, d'un système de causes inconnues et d'effets multiples)? Elle donne lieu simplement à des constatations de fréquence: certaines associations se produisent souvent, tandis que d'autres sont rares à des degrés divers. Mais si cette intuition de la fréquence constitue un début de l'intuition du mélange, elle n'en

est qu'une approximation incomplète et grossière. Elle est compatible avec d'autres éléments qui sont étrangers au concept du mélange et contradictoires avec lui, et elle les appelle même, à défaut de schèmes opératoires suffisants: nous avons vu combien les sujets du stade I croient à l'intervention, malgré le brassage, de qualités ou de pouvoirs intrinsèques des objets mélangés (le rose attire le rouge, etc.) et même de rapports inhérents à l'ordre que présentaient ces objets avant le brassage (ordre de l'échelle servant d'aide-mémoire). Pour que se forme une notion exacte du mélange, il est donc nécessaire que deux conditions soient remplies, qui dépassent l'une et l'autre l'expérience pure et l'intuition des fréquences: il faut un tableau des combinaisons possibles, qui seules donnent une signification aux combinaisons observées réellement, et il faut un système de rapports quantitatifs entre les diverses catégories se combinant les unes avec les autres et l'ensemble des combinaisons possibles. Il faut donc, dans les deux cas, qu'un système d'opérations soit élaboré, qui dépasse le donné et qui puisse l'assimiler à des schèmes proprement opératoires.

Nous avons vu comment le passage du stade I au stade II, c'est-à-dire de l'intuition des simples fréquences empiriques interprétées subjectivement à l'idée du mélange et au début des estimations probabilistes correctes, était précisément caractérisé par l'élaboration de certains schèmes combinatoires. En premier lieu le mélange lui-même est conçu comme un système de combinaisons fortuites, se manifestant par les diverses associations possibles entre les éléments brassés. En second lieu, nous avons surtout constaté comment le sujet se représente l'action du tirage comme liée à la valeur quantitative des catégories d'éléments en jeu et commence ainsi, aussitôt acceptée l'idée du mélange à quantifier les probabilités elles-mêmes. Ainsi intervient dans les deux cas, une sorte de schème opératoire anticipateur, dont nous retrouverons le rôle dans la construction même des opérations de combinaisons.

Au stade III, enfin, les intuitions cessent d'être globales et, tant l'affinement de l'idée de hasard que le calcul des probabilités, conçues comme des fractions de certitude par rapport à la totalité des combinaisons possibles, montrent l'achèvement du système opératoire lui-même qui permet de reconstituer ces combinaisons. De cette manière, l'intuition du mélange, qui

constitue au stade II la notion antithétique de l'idée de „groupement" opératoire, finit par être conçue sur le modèle du groupement lui-même, à cette seule différence près, qui oppose précisément l'idée rationnelle du hasard à celle de détermination simple, que seule la totalité (grands nombres) est déterminée (par le groupement multiplicatif complet des combinaisons possibles), les raisonnements portant sur les fractions demeurant „polyvalents".

QUELS SONT LES DETERMINANTS DE LA PREGNANCE PERCEPTIVE?

par

H. PIERON (Paris)

1. *La notion de prégnance et la théorie gestaltiste.*

Toute perception est une interprétation d'un complexus stimulateur qui intervient dans la régulation du comportement en déterminant une attitude réactive. Or, dans bien des cas, le même complexus stimulateur peut susciter des interprétations différentes, être différemment perçu.

Un facteur essentiel dont dépend l'interpretation réside dans l'orientation générale, à ce moment, des intérêts, des préoccupations, des tendances, qui entrainent une identification élective de ce qui répond à cette orientation. Un autre facteur, expliquant les différences perceptives d'un individu à l'autre, et surtout d'une espèce animale à l'autre, même avec une orientation commune de la conduite, tient à des inégalités de la structure réceptrice et des capacités sensorielles: à la recherche du même gibier, le chasseur et son chien n'auront pas des perceptions identiques.

Mais, n'y-a-t-il pas, dans la structure du complexus stimulateur des particularités susceptibles de provoquer la prédominance d'un certain type d'interprétation? C'est ce qu'a mis en évidence, au cours de nombreuses recherches, poursuivies surtout dans le domaine visuel, l'école de la Gestalt, analysant des complexus et les faisant varier systématiquement, dans des conditions d'ailleurs assez abstraites. Koffka, Köhler, Wertheimer, ont, les premiers, dégagé des «lois de prégnance» perceptive, et ont entendu les interpréter.

Wertheimer fait appel aux lois de proximité, de ressemblance, de disposition objective, de direction commune, de bonne forme, en combinant les arrangements les plus simples de lignes et de points [1]).

L'étude des conditions dont dépendra une perception de mou-

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[1]) Max Wertheimer. Untersuchungen zur Lehre von der Gestalt. *Psychologische Forschung*, IV, 1923, p. 301—350.

vement apparent, point de départ de l'ensemble des recherches, permettait déjà de préciser des lois dans les relations spatio-temporelles des stimulations inductrices, et une explication en est dégagée. Lorsque deux points se trouvent projeter leur image successivement sur des territoires corticaux voisins dans une marge d'écart qui grandit jusqu'à une certaine limite avec l'intervalle temporel, il s'effectue, entre ces territoires un court circuit entrainant l'excitation des territoires interposés, tout comme si l'image avait effectivement parcouru ces territoires au cours du mouvement réel d'un point entre les positions extrêmes [2]). Hypothèse séduisante, dont le retentissement fut grand, hypothèse à laquelle se rattache la ligne générale des explications de la prégnance.

Ces explications, Koffka et Köhler les rattachèrent au principe général de l'*isomophisme*, de l'identité structurale du monde physique et de l'appareil cérébral, à la communauté des lois d'organisation spatiale, d'équilibration dynamique régissant les phénomènes physiques et les processus physiologiques [3]). La forme prégnante aurait une valeur quasi universelle, indépendante presque entièrement de l'expérience. Il y a là une conception théorique très générale qui, si l'on ne veut pas rester sur un terrain métaphysique, implique des conséquences au point de vue des modalités du fonctionnement nerveux.

2. *Le fonctionnement perceptif cortical.*

Il est évidemment légitime d'envisager le fonctionnement nerveux comme celui d'un appareil électrique — ou du moins électrochimique — soumis à des lois physiques. L'excitation susceptible de déclencher des influx, l'inhibition qui les arrête ou les empêche, paraissent bien essentiellement relever de modifications de potentiel corrélatives de migrations ioniques.

Mais, si les mécanismes en jeu peuvent être clairement éluci-

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[2]) Cf. Wertheimer. Experimentelle Studien über das Sehen von Bewegung. *Zeitschrift für Psychologie*, LXI, 1912, p. 161—265. — Korte. Kinematoskopische Untersuchungen. *Idem*, LXXII, 1915, p. 193—321. — Koffka. Zur Theorie einfachster gesehene Bewegungen. *Idem*, LXXXII, 1919, p. 257—292. — Köhler. Zur Theorie der stroboskopischen Bewegungen. *Psychologische Forschung*, III, 1923, p. 393—406.

[3]) Cf. W. Köhler. An aspect of Gestalt Psychology. *Pedagogical Seminary*, XXXII, 1925, p. 691—723 — — *Gestalt Psychology*, 1929. — K. Koffka. *Principles of Gestalt Psychology*, 1935.

dés, il n'en va pas de même des modifications adaptatives qui se manifestent dans les processus cérébraux, comme dans tous les processus biologiques en général.

En mettant en évidence très justement le rôle des attitudes qui modifient la structure perçue, Koffka [4]) peut bien attribuer aux processus physiologiques sous-jacents la responsabilité de cette structure variable, mais il n'indique en aucune manière comment se peut expliquer physiquement l'attitude et ses conséquences, et, de façon générale la régulation du comportement dont elle est une manifestation.

Réduire le psychologique au biologique est parfaitement possible, mais le réduire au physique ne le parait guère, alors que le biologique ne peut actuellement s'y ramener entièrement.

D'autre part, en ce qui concerne même les mécanismes en jeu, les hypothèses purement physiques restent trop loin des faits pour avoir une réelle valeur, en ce qui a trait aux processus cérébraux dont la complexité est extrême. Nous voyons bien qu'il se produit des phénomènes de diffusion des potentiels dans la masse cérébrale, des interactions, qui entrainent des synchronisations dans les pulsations électriques que l'on sait aujourd'hui recueillir, et dont on peut concevoir, comme l'a fait A. Fessard [5]), des mécanismes purement physiques. Mais ce sont là des processus qui ne se manifestent pas dans les régulations fines du comportement normal, toujours physiquement imprévisibles.

Nous entrevoyons peut-être une èlucidation satisfaisante des activités réflexes élémentaires au niveau de centres relativement peu complexes, lorsqu'ils sont soustraits aux régulations supérieures.

Mais, lorsqu'on rend compte par un court-circuit électrique de l'impression apparente de mouvement — très apparentée à l'impression de causalité perceptive que Michotte a si bien mise en évidence et analysée [6]) — on se fonde sur une conception vraiment trop simpliste des mécanismes cérébraux.

[4]) K. Koffka. An introduction to the «Gestalt-Theorie». *Psychological Bulletin*, XIX, 1922, p. 531—585.

[5]) A. Fessard. Hypothèse sur le mécanisme d'intervention du facteur électrique dans la synchronisation interneuronique. *C.R. de la Société de Biologie*, t. 136, 1942, p. 268.

[6]) A. Michotte. *La perception de la Causalité. Etudes de Psychologie*, VI, Louvain, 1946.

Sans discuter même la valeur explicative — qui ne s'accorde guère avec l'absence complète de court-circuit dans les excitations simultanées — une expérience cruciale m'a permis depuis longtemps déjà de montrer qu'elle est directement contredite par les faits: En projetant une première image dans la région gauche du champ visuel de l'oeil gauche, et la seconde dans la région droite du champ visuel de l'oeil droit, ou inversement, on provoque de la même manière le mouvement apparent [7]), le phénomène phi, que dans les conditions normales de la vision binoculaire alors que la réception se fait dans deux rétines différentes (ce qui élimine toute possibilité de court-circuit oculaire) et dans deux hémisphères cérébraux différents (ce qui élimine toute possibilité de court-circuit cérébral).

La même expérience a été reprise depuis lors avec la même conclusion relative à l'impossibilitè du court-circuit.

Il est cependant possible que les réactions engendrées par des stimulations successives dans les conditions où l'on perçoit un mouvement apparent soient conditionnées par des relations nerveuses préformées et que l'on peut dès lors qualifier de structurales.

Il en est même à coup sûr ainsi lorsque la rèaction est constituée par le réflexe optocinétique sous-cortical, tel que l'ont étudié Smith et Kappauf [8]) chez le chat, qui prèsente encore un nystagmus stroboscopique — moins précis d'ailleurs — après destruction de l'aire striée réceptrice du cortex. Mais la perception du mouvement ne conditionne pas nécessairement le réflexe comme l'affirment les auteurs, perception qui, chez le chat, exige l'intervention corticale. C'est beaucoup plutôt le réflexe, quand il est provoqué, qui peut contribuer à engendrer la perception de mouvement chez l'animal normal. Mais on sait que, chez l'homme, le mouvement apparent peut-être perçu en dehors des mouvements oculaires; deux mouvements de sens inverse pouvant être simultanément perçus. Or, au niveau du cortex, quel est le rôle de structures préformées? C'est là une question fondamentale. L'expérimentation physiologique sur les

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[7]) H. Pieron. Remarques sur la perception du mouvement apparent (à propos des théories gestaltistes). *Année Psychologique*, 34e année, 1934, p. 245—248.

[8]) K. U. Smith et W. E. Kappauf. A neurological study of apparent movement. *Journal of general Psychology*, t. *23*, 1940, p. 315—327.

Primates, l'analyse des données de la neuropathologie, le développement de la chirurgie cérébrale et l'enregistrement des manifestations électriques de l'activité du cerveau ont déjà permis de déterminer, dans ses lignes essentielles, les modalités du fonctionnement de la sphère corticale dont l'architectonie a précisé la structure.

La notion fondamentale qui se dégagé est celle de variabilité et de plasticité.

En dehors des zones d'insertion des voies afférentes, apportant les messages des appareils récepteurs et des centres sous-jacents d'un côté, des zones de départ des ordres moteurs ou des incitations régulatrices de l'autre, avec leurs connexions plus ou moins directes, il existe un immense feutrage de prolongements cellulaires susceptibles d'établir les connexions les plus variées en lesquelles s'inscrivent les expériences qui modéleront la pensée au cours du développement mental, régi par les influences éducatives et sociales. Il existe bien dans le cortex humain quelques structures préformées dont dépendent des réflexes de fine régulation, exigeant l'intervention des «analyseurs» que constituent les esthésiones des zones réceptrices du cortex: certains réflexes oculaires ont ainsi émigré, chez l'homme, dans le cortex cérébral, et c'est là que siège le substrat des régulations réflexes de la vision binoculaire en profondeur.

Mais l'énorme développement pris par le néencéphale dans l'espèce humaine est corrélatif de la substitution croissante, aux mècanismes tout prèparès pour le comportement rigide des espèces inférieures, d'une capacité d'acquisitions assurant une plasticité adaptative aux circonstances variables de la vie individuelle. Il s'est formé des terres vierges qui seront ensemencées progressivement, et dont le rendement s'accroîtra en fonction de leur utilisation même, leur stérilisation risquant de se produire lorsqu'elles sont laissées en friche. Elles peuvent présenter, de par leur situation, des électivités qui les prédisposent à certaines grandes fonctions, comme c'est le cas pour le langage [9]).

[9]) Un hémisphère dominant se spécialise pour les régulations motrices de la main, et en même temps, de façon presque constante (environ 94 % des cas), pour les perceptions et les commandes expressives du langage). Mais, dans les cas de destructions partielles, des suppléances peuvent s'établir, manifestant la plasticité du cortex. Gowers avait déjà signalé que, chez les enfants, les lésions de l'hémisphère dominant n'entrainaient jamais d'aphasie durable.

Les prolongements cellulaires, de longueur plus ou moins considérable, qui constituent le réseau dans lequel les processus nerveux vont assurer le fonctionnement mental, ne se développent que peu à peu après la naissance, où l'individu humain donne un modèle de comportement en l'absence de cortex. De Crinis a constaté que la région du pied de la 3e frontale, normalement mise en jeu pour les expressions verbales dans la fonction du langage, ne commençait à présenter des expansions dendritiques cellulaires que vers 14 mois, et ne les achevait que vers 6 ans [10]). Si, dans cette période un enfant est placé dans des conditions où il ne peut acquérir le langage (tous les cas connus d'enfant «sauvages»), il ne pourra ensuite apprendre que très difficilement et très incomplètement à parler. Privés de l'usage de la vue à la naissance, de jeunes singes à qui on ne rend cet usage qu'après deux années, ne peuvent pratiquement plus acquérir de perceptions visuelles.

L'activité perceptive, dans sa souplesse discriminative, est une fonction essentiellement corticale; elle se développe en des territoires normalement voisins des stations de réception des messages, dans la sphère corticale, sur la base de l'expérience.

Mais comment s'acquiert l'expérience? On a trop souvent tendance à envisager, comme facteur d'acquisition, la répétition simple des stimulations, en invoquant ce qu'on a appelé, d'un terme impropre — nous y reviendrons — la «familiarité» comme facteur de prégnance perceptive. Et des expériences ont montré que ce facteur de répétition pouvait se montrer inefficace, d'où l'on concluait que l'expérience ne jouait pas de rôle [11]). Mais la conclusion est illégitime, car l'acquisition de l'expérience exige

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[10]) Max de Crinis. Die Entwicklung der menschlichen Grosshirnrinde in ihrer Beziehung zur geistigen Entwicklung. *Ienaischer Zeitschr. für Medizin*, t. 76, 1943, p. 322—340.

[11]) Ce sont, en particulier les expériences de Gottschaldt, qui se contentait de répéter des présentations pour fixation mnémonique. Mais en outre il cherchait à faire découvrir une figure partielle dans un complexe, ce qui change complètement les conditions perceptives toujours adaptées à un ensemble. La capacité de décomposer un ensemble de différentes manières correspond à une aptitude spéciale, la «flexibility of closure» de Thurstone, très variable d'un individu à l'autre, et son intervention n'est pas faite pour mettre en évidence des lois générales de la perception (Cf. K. Gottschaldt — Ueber den Einfluss der Erfahrung auf die Wahrnehmung von Figuren. *Psychologische Forschung*, t. 8, 1927, p. 261—317).

un mécanisme bien déterminé, celui de conditionnement, qui implique la loi générale de l'effet.

3. *Le rôle du conditionnement adaptatif* [12]).

Un stimulus *a* précédant habituellement une excitation réflexogène peut bientôt se substituer à celle-ci pour entrainer les mêmes effets, pour prendre même signification, même valeur; mais si, quand il s'accompagne d'un autre stimulus *b*, il n'est plus jamais associé à l'excitation efficace, il perd des lors cette signification, et la présence de *b* est dite inhibitrice. En réalité alors que *a* prend une signification perceptive, le complexe $a+b$ en prend une différente. Ce sont deux structures dont la signification s'acquiert par expérience, à la suite de déceptions ou de confirmations répétées.

Il naît ainsi des comportements d'anticipation [13]). Mais, ce qui donne signification, ce n'est pas la répétition d'une stimulation, mais celle d'un complexe dans lequel le stimulus est suivi d'un certain effet, et c'est l'effet qui joue le rôle essentiel dans la signification. Si, chez les animaux inférieurs, le conditionnement, loi générale de l'adaptation vitale, peut se manifester, même en l'absence de systèmes nerveux, chez les Primates et surtout chez l'homme, le conditionnement est réservé à l'appareil de régulation de la conduite, au cortex.

Les expériences relatives aux effets d'une inversion dans les réceptions oculaires mettent bien en évidence les possibilités accrues de plasticité perceptive qu'assure le développement du télencéphale.

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12) J'ai déjà soutenu, en 1927, que les lois de la perception se ramenaient à celles du conditionnement, correctement formulées et décelables dans les expériences de dressage, comme celles de Révész que je citais (Experimental study in abstraction in Monkeys. *Journal of Comparative Psychology*, t. 5, 1925, p. 293—343, et relatives à des dissociations dans des perceptions de formes et de couleurs (Cf. H. Pieron. Les problèmes de la perception et la psychophysiologie. *Année Psychologique*, t. 27, p. 1—22).

13) J'ai mis en évidence, dès 1906, des processus d'anticipation chez des Invertébrés, des organismes inférieurs tels que les Coelentérés, et correspondant au mécanisme du conditionnement pavlovien dégagé au niveau du cortex chez les Mammifères (Cf. H. Pieron. La réaction aux marées par anticipation réflexe chez *Actinia equina*. *C.R. Société de Biologie*, t. 61, 1906, 658—660. Des phénomènes d'adaptation biologique par anticipation rythmique. C.R. *Académie des Sciences*, t. 144, 1907, p. 338—341).

Les expériences de Stratton, reprises par Ewert [14]), sur l'effet du redressement des images rétiniennes, ont nettement montré la possibilité du rétablissement d'une vision droite au bout d'un apprentissage par essais et erreurs, d'une durée de 14 jours dans les expériences d'Ewert, avec mise en évidence du rôle capital de l'«effet».

L'inversion spéculaire, avec emploi de lunettes prismatiques est encore plus facilement corrigée, et l'on a déjà en quelque sorte, lorsqu'on a l'habitude de se servir du miroir, un double clavier régulateur, assurant, par commutation, l'adaptation à l'inversion [15]). Mais G. G. Brower a montré que l'on corrigeait même des déviations de valeur anormale, inclinant à 75° l'horizontale [16]). Au bout de 7 jours, si le redressement perceptif des verticales inclinées n'était pas encore complet, du moins pour la vision proche, dans le champ des activités habituelles, la régulation était déjà à peu près complète, au cours de ses expériences.

Cette plasticité, que Foley Jr a également observée chez le macaque pour le redressement, à 180°, en une semaine, des images rétiniennes [17]) caractérise les réactions commandées par le cortex et les perceptions corrélatives, mais fait entièrement défaut pour les réflexes que Sperry a étudiés chez des Vertébrés inférieurs, chez divers Batraciens, salamandres, crapauds, après avoir réussi à réaliser une inversion oculaire de 180° [18]).

[14]) P. H. Ewert. A study of the effect of inverted retinal stimulation upon spatially coordinated behavior. *Genetic Psychology Monographs*, t. 7, p. 179—363).

[15]) K. Lewin en 1923 a établi la possibilité d'un redressement perceptif de mots présentés à l'envers, après une période d'apprentissage de lecture de ces mots, défilant sur une bande, et conclu que la perception dépendait des processus psychiques dans lesquels elle était englobée et n'était pas directement conditionnée par la structure des processus rétiniens (Ueber die Umkehrung der Raumlage auf dem Kopfstehender Worte und Figuren in der Wahrnehmung. *Psychologische Forschung*, 4, 1923, p. 210—261).

[16]) G. G. Brower. Perception of depth with disoriented vision *British Journal of Psychology*, t. 19, 1928, p. 117—146.

[17]) J. P. Foley, Jr. Observations on the effect of prolonged inverted retinal stimulation upon spatially coordinated behavior in the Rhesus monkey (*Macaca mulatta*) *Psychological Bulletin*, t. 35, 1938, p. 701—702.

[18]) R. W. Sperry. Optic nerve regeneration with return of vision in Anourans. *Journal of Neurophysiology*, t. 7, 1944, p. 57—69. L'insertion de l'oeil retourné est faite après section du nerf optique, qui régénère en gardant la même signification locale des messages transmis.

Jamais les réactions des animaux n'ont cessé de se faire en direction juste opposée à celle qu'aurait exigée la position spatiale des stimuli: réflexes optocinétiques, essais de préhension des proies. La seule correction adaptative a consisté dans l'inhibition complète des réponses qui étaient constamment suivies de déception: Les crapauds renonçaient à essayer de saisir les mouches et mouraient de faim si on ne les nourrissait pas artificiellement, périssant ainsi par suite de la permanence de leurs structures congénitales réactives.

4. *Les données expérimentales et l'analyse des conflits perceptifs.*

Le mouvement apparent, dont l'analyse, par Wertheimer a constitué le point de départ de la psychologie de la Gestalt, se montre tout particulièrement favorisé par les conditions qui reproduisent le mieux des expériences usuelles et s'accordent davantage avec l'expérience fondamentale de la permanence des objets. Un ballon perçu en un point puis un ballon identique perçu en un autre point, ne suggère pas l'évanouissement du premier et la création subite de l'autre, mais un déplacement du même ballon, assez rapide pour n'avoir pu être suivi de l'oeil. Et le mouvement apparent d'un ballon sera perçu avec une tolérance bien plus large pour les relations d'écart spatial ou temporel que celui d'un point abstrait ou de l'image d'une maison. Comme l'a fait remarquer de son côté Erismann [19]) le mouvement implique quelque chose qui se meut, et ce quelque chose a dans notre expérience une permanence que de multiples conditionnements ont solidement imprimée.

Mais, entre les objets, même quand nous savons qu'on ne nous en présente que des images dont le jeu conventionnel est plus souple, nous sommes bien plus disposés, dans les mêmes conditions perceptives; à voir se déplacer ceux dont nous connaissons la mobilité, ainsi que Krolik l'a précisé [20]).

Et, en outre Krolik, en montrant une image immobile d'un ballon pendant que celle d'un train se déplace vers le bas et la

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[19]) Th. Erismann. Wesen und Entstehung der Scheinbewegung. *Archiv für die gesante Psychologie*, t. 100, 1938, p. 305—386.

[20]) W. Krolik. Ueber Erfahrungswirken beim Bewegungssehen. *Psychologische Forschung*, t. 20, 1934, p. 47—101.

droite, obtient une perception d'un double mouvement, celui d'un ballon qui monte et celui d'un train qui glisse horizontalement vers la droite, en accord avec les évènements habituels.

Quand les données d'expérience entrent en conflit avec des données sensorielles déterminées, la prépondérance se marquera souvent pour les premières — mais avec une grande variabilité individuelle — si elles sont plus solidement établies. L'intérêt du mouvement apparent est de permettre de graduer, par le jeu des écarts de lieu et d'instant, la force des données sensorielles élémentaires susceptibles de s'opposer à l'induction perceptive de mouvement, et par conséquent d'apprécier la force de l'induction acquise. En principe, on devrait pouvoir, chez les enfants, suivre quantitativement le progrès de l'éducation perceptive. Mais il faudrait obtenir une manifestation objective valable de cette induction. Les expériences faites sur les enfants sont en général d'interprétation assez difficile quand elles comportent des réponses verbales, et impossible aux premiers stades [21]).

Les analyses de Piaget paraissent bien cependant s'opposer à la préformation des structures, en particulier pour les constances perceptives. Et l'on pourrait utiliser aussi les cas d'arrêt de développement mental. On sait déjà que l'illusion musculo-visuelle de soupèsement, très forte chez les adultes normaux, fait à peu près défaut chez les imbéciles; elle se base sur la surprise de trouver plus lourd un objet plus petit qu'un autre semblable et beaucoup plus volumineux, à l'encontre de l'expérience conditionnante habituelle enregistrée dans un cortex normal.

Toutes les constances perceptives, celles des qualités réflexives des surfaces d'objets — plus claires ou plus sombres indépendamment de l'éclairage, et de couleur propre déterminée malgré des compositions variables de la lumière réfléchie [22]) —

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[21]) Cf. E. Schroff. Ueber Gestaltauffassung bei Kindern im Alter von 6 bis 14 Jahren. *Psych. Forschung*, t. 11, 1928, p. 235—266.

Grace Preyer Rush. Visual grouping in relation to age. *Archives of Psychology*, n°. 217, 1937.

[22]) Les aspects visuels des surfaces sont des complexes perceptifs qu'on a longtemps confondus avec des sensations élémentaires. En isolant la surface examinée, on ne voit plus de gris ni de noirs, de roses, de marrons ou de bruns. On n'a de ces perceptions de *leucies* et de *chromoleucies* que

celles de grandeur et de forme, indépendamment des perspectives et des distances, constituent également des manifestations de l'expérience fondamentale d'une permanence des objets, rarement contredite.

Ce sont aussi des facteurs dont on peut évaluer chez un individu donné la force propre par la méthode des conflits.

Du fait que certaines structures réflexogènes sont réellement inscrites, congénitalement, dans les centres encéphaliques, on peut artificiellement contredire les interprétations normales de ces structures en faisant appel à des données déterminées de l'expérience courante; le degré de prépondérance de ces dernières permet de mesurer leur force, de comparer des individus, des attitudes, et des ensembles perceptifs voisins.

C'est cette méthode que nous avons, avec Mme Bernyer et Durup, utilisée, en mettant en conflit une variation de dimension de l'image rétinienne d'un objet ou d'un autre avec sa distance apparente résultant du point de convergence des yeux réalisant l'unification binoculaire des images [23]).

Alors que, pour l'examen discontinu d'un objet peu significatif, tel qu'une règle, avec une marge limitée des variations de distance du plan de convergence, les appréciations de dimension et de distance peuvent être correctes, bien que l'objet plus proche soit vu sous un angle plus petit, il n'en est plus de même si l'on prend un objet plus familier, si l'on augmente la marge, et surtout si l'on fait suivre l'objet dans son déplacement, qui entraine la discordance. Les inégalités dans la marge compatible avec les appréciations correctes, dans le premier cas, caractérisent déjà les individus différents. Et l'on a des différences indi-

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dans des complexes perceptifs. Au point de vue des termes à employer pour leur désignation, voir: H. Pieron. La terminologie visuelle. *Année Psychologique*, t. 41—42, 1945, p. 248—251.

[23]) Cf. G. Bernyer, G. Durup et H. Pieron. Contribution à l'étude des conflits perceptifs, *Année Psychologique*, t. 40, 1940, p. 15—51. La méthode employée a été celle des ombres colorées en vision anaglyphique: une source de lumière rouge et une de lumière verte éclairant un écran, un objet interposé donne deux ombres chevauchant dont la vision est séparée par emploi d'un lorgnon bicolore. Chaque oeil, percevant l'ombre décalée de son côté, la convergence doit diminuer pour réaliser l'unification visuelle quand les ombres s'écartent pour que l'objet se rapproche des sources; mais en même temps les images sur l'écran s'agrandissent. L'objet doit paraître s'éloigner, mais doit aussi paraître s'agrandir, d'où le conflit.

viduelles dans les erreurs d'évaluation de la grandeur d'un côté, de la distance de l'autre, quand s'établit le compromis. La prépondérance appartient normalement, en cas de mouvement, aux variations de grandeur de l'image rétinienne, c'est-à-dire à la constance de cette grandeur — du moins pour certains objets — au point de faire apparaître comme se rapprochant un objet alors que la convergence diminue, et s'éloignant alors que la convergence augmente, à l'inverse de l'interprétation fondée sur la structure réflexogène (l'objet étant localisé au niveau du plan de convergence, suivant la loi de Giraud–Teulon).

La pseudoscopie permet aussi de mettre en conflit les données de l'expérience avec les interprétations perceptives des disparations rétiniennes qui constituent des structures réflexogènes, commandant les régulations accommodatives et coordinatrices de la motricité binoculaire.

On sait qu'il est impossible de voir inversé le relief d'une figure humaine, et j'ai étudié les conditions dans lesquelles des images stéréoscopiques pouvaient, en vision pseudoscopique, comporter une inversion effective des effets de profondeur. Là encore la variabilité interindividuelle et intraindividuelle est notable. On voit souvent surgir une inversion par une interprétation vraisemblable de l'image, comme cela se produit avec les figures réversibles, et l'on peut susciter l'inversion en suggérant l'interprétation.

Un exemple que j'ai souvent utilisé dans des démonstrations est celui d'une butte de terre sur laquelle grimpe à quatre pattes un personnage, situé à peu près à mi-hauteur. Une interprétation de compromis peut brusquement survenir: Au dessous du personnage, la saillie de la butte se transforme en caverne, cependant qu'à son niveau et au-dessus on continue à percevoir la butte saillante.

En pseudoscopie d'objets réels, par inversion avec le jeu des doubles miroirs, un cylindre plein peut-être vu comme un hémicylindre en creux; si au dessus de lui on fait apparaître une tige verticale située réellement en avant, on la percevra bien en arrière, mais, si l'on fait descendre la tige devant le cylindre, brusquement et impérativement, on perçoit dans leurs relations correctes le cylindre redevenu plein, et la tige, malgré les disparations inversées. Les conditionnements acquis l'emportent nettement sur les structures.

5. *Rôle de l'économie d'acquisition.*

Peut-on ramener à l'influence de l'expérience la prégnance constatée des formes simples, symétriques et régulières? Si l'angle droit a une incontestable prégnance, on peut effectivement constater que, dans les objets fabriqués, meubles, boites, édifices, etc., les angles droits étant constamment la règle, on en possède une expérience courante. Les roues, les cadrans, etc., mettent le cercle dans notre ambiance familière. Mais on peut faire intervenir un autre facteur qui, pour l'acquisition même de l'expérience, joue un rôle incontestable, celui de l'économie.

Dans l'extrême diversité des évènements, nous facilitons l'enregistrement par une schématisation simplifiante, compatible avec un nombre limité de symboles verbaux. Nous avons trois mots pour désigner une verticale, une oblique, une horizontale; alors que l'oblique reste assez indéterminée, la verticale et l'horizontale sont bien définies, et on a plus de chance d'une approximation satisfaisante en rattachant à la verticale ou à l'horizontale des positions assez voisines, comme en rattachant à l'angle droit, bien défini, des angles faiblement aigus ou obtus, recouvrant une large indétermination. Le cercle est bien défini par rapport à la multiplicité des ellipses ou des polygones, le carré par rapport à la multiplicité des rectangles, et plus encore à celle des quadrilatères.

Lorsque nous avons à retenir, à reproduire une forme, l'économie est grande si nous pouvons rattacher cette forme à un cercle, à un carré. Dans la reproduction des mouvements imposés, plus ou moins complexe, on voit s'établir aussi une régularisation simplificatrice qui répond à cette grande loi de l'économie [24]).

Nous accentuons des caractéristiques qui facilitent la désignation, le classement des objets en catégories de nombre limité, l'assimilation du nouveau, de l'inhabituel au connu, à l'usuel, à l'enregistré, qui, par là même se trouve davantage répété, davantage reproduit, et devient plus familier.

Ainsi le principe d'économie, par ses effets mêmes, vient accentuer le principe de familiarité.

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[24]) Dans les travaux dirigés par Michotte, voir en particulier celui de G. de Montpellier: Les altérations morphologiques des mouvements rapides. *Etudes de Psychologie,* volume IV, Louvain, 1934.

Conclusion

P. Guillaume, en exposant avec sympathie l'ensemble des travaux relatifs à la psychologie de la forme, n'a pas manqué de signaler l'existence de types de prégnance relatifs aux objets des instincts, et c'est à cette forme que Mc Dougall entend ramener le fondement de toute perception dans sa conception «hormique» générale [25]). Mais Guillaume reconnait que «les lois de simplicité, de régularité, de symétrie semblent être faites pour des formes géométriques ou musicales et sont, en fait, admirablement illustrées par des exemples de cet ordre» [26]).

En dehors de la prégnance des formes qui correspondent aux intérêts naturels ou artificiellement développés (la figure humaine — qui s'impose dans les interprétations de nuages, de taches d'encre, ou de jeux de la nature comme on en signale tant d'exemples dans des aspects des nuages ou des rochers — les lettres et les mots, etc.) pouvons-nous admettre une prégnance attribuable à des structures nerveuses préformées, étant entendu que les prégnances acquises reposent sur des structures dynamiques progressivement établies? La prégnance d'une saillie ou d'un creux dans des images stéréoscopiques présentées avec une disparation rétinienne directe ou croisée repose certainement sur une structure congénitale, se traduisant en des réflexes, en dehors de toute expérience. Mais on peut penser que ce sont des structures qui ne sont pas physiquement conditionnées par un isomorphisme, et qui se sont fixées par un processus adaptatif.

En tout cas nous voyons, par la méthode des conflits, que ces structures, qui sont modifiables (dans l'inversion prolongée des images visuelles) peuvent être contrebalancées dans leur interprétation perceptive, par d'autres structures certainement acquises.

Je suis entièrement convaincu que les seuls facteurs de prégnance perceptive, dans le fonctionnement cortical humain, se ramènent, tout d'abord à la *familiarité*, mais en entendant par là une conséquence de conditionnements répétés dans lesquels joue la loi de l'effet, et non une simple fréquence de stimulation,

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[25]) W. McDougall, Dynamic principles of Gestaltpsychology. *Character and Personality*, t. 4, 1936, p. 232—244 et 319—334, et t. 5, p. 61—82 et 131—148.

[26]) P. Guillaume. *La Psychologie de la Forme*, Paris, 1937, p. 227.

et, en second lieu à *l'économie* dans l'enregistrement de l'expérience, qui explique le rôle de la simplicité, de la symétrie, de la régularité, et qui, par suite de l'évocation usuelle des schémas enregistrés et de la reproduction dans les oeuvres humaines, entraine, secondairement, la familiarité plus grande des formes simples, régulières et symétriques, ainsi doublement favorisées.

Les structures corticales commandant les processus perceptifs sont corrélatives des conditionnements, qui représentent le mécanisme normal de l'adaptation par expérience dans tout organisme vivant.

QUELQUES RÉFLEXIONS SUR LE PROBLÈME DE L'ORIGINE DU LANGAGE

par

H. J. Pos (Amsterdam)

L'auteur de cet article a vivement accepté l'invitation d'offrir à GEZA RÉVÉSZ, à l'occasion de son soixante-dixième anniversaire, un témoignage de sympathie amicale et intellectuelle. Parmi les très nombreux sujets que ce psychologue de grande envergure a traités pendant une carrière de bientôt cinquante ans, il en figure un qui a été mon premier amour intellectuel et qui pendant de longues années est resté le principal objet de mes efforts et de mes réflexions: le problème de l'origine du language. RÉVÉSZ a consacré à ce sujet un livre remarquable paru en 1946. J'ai publié quelques brèves reflexions sur les thèses de ce livre dans le Algemeen Nederlands Tijdschrift voor Wijsbegeerte en Psychologie sous le titre de: Oorsprongsproblemen (tome 39, année 1946/47, pp. 42—48). Là le problème de l'origine du langage avait été placé dans le cadre plus ample, qu'est celui de l'origine des phénomènes spirituels en général et quelques objections y avaient été soulevées au sujet de la façon dont les points de vue de la structure et de l'évolution avaient été synthetisées. Ces réflexions restant dans l'ordre épistèmologique et philosophique n'entraient pas dans les détails de la théorie proposée. Si aujourd'hui je propose quelques aspects de l'évolution probable du langage humain, ce n'est pas pour discuter philosophie avec le savant psychologue. C'est plutôt pour faire un travail complémentaire où je tâche de montrer quelques apports de l'histoire des langues utiles à la théorie de son origine. Je m'y rapproche beaucoup plus des thèses de l'auteur du Problem des Ursprungs der Sprache et il verra lui-même à quel point j'ai appris de lui. J'espère qu'il trouvera autant plaisir à connaître mes vues et à s'y reconnaître que j'ai eu moi-même d'instruction en étudiant les siennes.

Le traitement d'un problème doit commencer par la constatation d'un fait ou d'un groupe de faits. Le langage est un fait qui

fait partie des faits humains. Sa grande importance consiste dans le rôle qu'il joue dans la vie humaine. Il a plusieurs fonctions, qu'on ne saurait réduire à une seule. On se rend compte de l'importance du langage surtout quand un besoin se trouve être insatisfait ou quand la satisfaction en est entravée. On a souvent le besoin de faire connaître un désir. Imaginons un homme qui va faire des achats dans un pays dont il ne connaît pas la langue. Il pourra indiquer par la main les objets qu'il désire acheter, mais aussitôt qu'il s'agit d'en indiquer le poids il sera gêné. Et il en sera de même pour le prix que lui dira le vendeur. Chacun connait la situation de malaise de celui qui ne connait la langue du pays. Il tache d'exprimer par des gestes, en imitant et en mimant les objets de ses désirs. Cette substitution même de la parole par les signes révèle à la réflexion un aspect du langage: c'est un système de signes, mais un système priviligié, qui a plusieurs avantages par rapport aux signes plus ou moins naturels qui nous servent là où le langage fait défaut. La différence consiste en ce que le langage avec un effort sensiblement moins grand met à notre disposition des signes couvrant un rayon beaucoup plus étendu que l'imitation par gestes.

Voici donc une première fonction du langage humain, l'expression des besoins en vue de leur satisfaction, autant que celle-ci dépend de l'aide des autres êtres humains. La conscience d'un besoin consiste toujours en un état d'inquétude et d'insatisfaction, mais elle n'y consiste pas exclusivement. Il y a toujours dans cet état un élément de réprésentation qui dépasse la tension du besoin et est libre par rapport à elle jusqu'à un certain degré. Le besoin n'est jamais tout à fait aveugle, il vise quelque chose, et souvent il se souvient de ce qui l'a rempli antérieurement, aussi veut-il, au moment présent, quelque chose d'analogue. C'est que la conscience du besoin n'est pas sans une certaine conscience de sa ressemblance à un besoin qui antérieurement a trouvé sa satisfaction. Il se reconnait par le souvenir et par là même il se généralise. L'idée générale sortie de l'expérience vécue repose sur le souvenir de la situation analogue et c'est là un élément intellectuel, qui se superpose au besoin réel et vécu. D'ailleurs le facteur intellectuel qui accompagne tout besoin humain ne se trouve pas seulement en le sujet qui ressent le besoin. Pour qu'il y ait un langage qui sert comme moyen à la satisfaction du besoin il faut que le partenaire, celui auquel s'adresse le

besoin en vue de sa satisfaction entende de quoi il s'agit. Par conséquent le langage ne pourra servir comme „signe du besoin" si ses expressions n'ont pas une valeur qui dépasse le sujet en question et qui s'étend aussi à celui qui écoute. C'est que le mot qui sert comme signal du besoin, pour être compris, doit avoir une signification qui dépasse non seulement la différence entre la situation présente et le besoin souvenu, mais aussi entre celui qui exprime le besoin et celui auquel il s'adresse pour en obtenir la satisfaction.

Nous voilà arrivés à un trait caractéristique de tous les signes du langage: leur généralité *objective*, qui pourtant n'est pas généralité *universelle*, puisque les signes sont compris seulement dans une communiuté linguistique, c. à d. par ceux parlant la même langue et non en dehors. Il est vrai que ceux qui parlent une même langue considèrent les mots comme les expressions naturelles des choses ou objets que ces mots signifient, mais ceci étant le cas pour toutes langues qui pourtant se distinguent par le fait que des mots tout différents servent pour exprimer la même chose, nous devons conclure que cette impression d'universalité de la propre langue est une illusion. Cette illusion se range parmi nombre d'autres illusions. Nous sommes toujours portés à tenir pour naturel et même rationnel les habitudes, les sentiments, les opinions de la collectivité dans laquelle nous vivons, et il en est ainsi du langage. Nous ne servons pas seulement des mots comme de signes pour les pensées et les choses, le lien entre les mots de notre langue et les choses est tellement intime que ces mots nous semblent correspondre à leurs significations, et les copier en un sens.

Cette illusion a donné naissance à une première théorie de l'origine du langage, à laquelle nous devons nous arrêter. C'est la théorie du rapport naturel entre de signe, ou le mot et sa signification. Ce rapport naturel a déjà vexé les penseurs grecs, et notamment Platon, qui y a consacré son dialogue Cratyle. Ce très grand penseur a pris position envers le relativisme des sophistes qui expliquaient les mots par institution et tradition. Platon est un penseur religieux, qui dans toutes les choses humaines voit un fondement éternel. Il regardait avec adoration le splendide cosmos, dont les phénomènes dans leur magnifique regularité lui semblaient témoigner d'une intelligence divine qui a tout formé. Il voyait dans les lois qui règlent la conduite

humaine la manifestation des idées transcendantes qui sont la source même de toute connaissance et de toute existence. Aussi est il compréhensible que Platon cherchait à retrouver dans le langage les traces de cette expression naturelle des choses qui lui paraissaiet digne du législateur divin. Et puisque pour lui expression naturelle voulait dire expression cognitive, expression correspondant à la nature même de la chose nommée, il devait se mettre à la recherche d'un sens caché dans les mots. Il s'en tira en considérant les mots comme composés de sons et en attribuant à chaque son une certaine valeur expressive et cognitive. Hélas, la preuve de la composition rationelle du mot échoue. Le S est signe de mouvement, comme le preuvent *seio* et *seuo*, mais que dire de *skleros*, qui veut dire dur, raide? Platon renonce à la fin du Cratyle à l'explication rationelle des mots. Il couvre l'échec en disant que la connaissance des idées est d'une valeur tout à fait supérieure à celle des mots. Si Platon ne s'est plus occupé du problème de l'origine des mots, il ne l'a premièrement pas résolu. La recherche des fondements éternels des choses ne doit pas trop s'approcher des choses dans leur contingence donnée. Le Cratyle de Platon en donne l'example. Dire que généralement et essentiellement le langage est rationnel, qu'il exprime l'ordre que l'esprit humain trouve ou établit dans les choses, qui le nierait? Qui nierait que le langage est en général le privilège d'un être pensant et qu'en lui les vues de l'esprit humain sur la réalité sont déposées? Qui pourrait nier l'apparente rationalité dans le tout, mais qu'il est impossible de retrouver dans les multiples détails? Prenez le langage comme un tout et il vous fournit des trésors de symboles. Mais considérez le dans le détail et vous remarquerez des défaillances, des contingences et des irregularités nombreuses.

Nous disions tout à l'heure que le langage est un moyen de manifestation des besoins vitaux, et que ce moyen a une structure générale et intellectuelle. La théorie de l'origine qui va de pair avec cette perspective du langage est la théorie évolutionniste. Elle enseigne que les hommes ont commencé par s'entendre sur leur besoins vitaux, la faim, la soif, la sécurité, l'appétit sexuel, aux moyens de sons produits par les organes vocaux. Les sons seraient sortis de gestes, de mimiques comme on les observe chez les enfants très jeunes. Ces gestes accompagnent les états de tension de l'organisme, la douleur, le bien-être, la surprise. Une

fois qu'ils se centralisent dans l'organe vocal, ils s'articulent et se raffinent et leur portée augmente. Le son atteint même ceux qu'on ne voit pas, il ne s'efface pas dans l'obscurité, il est moins fatiguant à produire que le geste qui demande un effort musculaire considérable, puis il donne à celui qui le produit une jouissance esthétique qui fait que l'acte de parler est un véritable plaisir à condition qu'il ne se prolonge pas trop. Quand on se réalise les différentes possibilités de symbolisme dont dispose l'organisme humain pour s'extérioriser, on y voit figurer tout ce qui est mobile. L'homme peut en un sens parler avec les pieds et déjà l'enfant le fait quand il est très en colère. Nos mains et nos bras sont capables d'un jeu très varié de gestes. Avec la main on peut commander, arrêter une personne, la faire approcher. La tête peut dire oui ou non, elle peut exprimer la résignation, la fermeté de décision. De que d'expressions le visage humain n'est-il pas capable! Que de sentiments la bouche et les yeux ne peuvent-ils pas trahir!

Et pourtant toutes ces expressions restent immédiates. Elles ne peuvent être aperçues et entendues que par des partenaires présents. Puis elles concernent toujours l'état immédiat dans lequel se trouve le sujet qui les exprime. Or, le langage dépasse les manifestations immédiates des tendances de l'individu: il nous met en état de pas seulement extérioriser un besoin ou un désir, mais d'en parler, de l'objectiver et par conséquent de l'intellectualiser. Nous entendons par intellectualisation la possibilité de se représenter un phénomène sans que ce phénomène corresponde à l'état présent du sujet humain. Quand par example je parle de la douleur que j'ai ressentie avant ou bien je parle de la douleur qu'un autre être ressent en ce moment, je ne m'identifie plus avec mon état passé, mais je m'y refère objectivement, comme je me refère objectivement à la douleur d'autrui ou son besoin, dont moi-même je ne suis pas le sujet. Cette possibilité d'avoir présent à l'esprit comme objet un phénomène que je pourrais subir par l'experience, mais que je ne subis pas est la base de l'intellectualisation. Elle est grandement favorisée par l'emploi du langage et elle est à la base de toute culture intellectuelle, qui consiste essentiellement dans la capacité de l'esprit humain de se libérer de la conscience de sa situation immédiate et de se diriger vers un objet ou un but en dehors de cette situation. Mais ce passage de la conscience

immédiate à la connaissance objective exige une modification appropriée de l'expressivité et qui consiste en ce que l'expression immédiate soit remplacée par un symbolisme non plus expressif, mais intelligent, et qui vient se superposer. Supposons que le langage ait été originairement expressif et que les êtres humains aient communiqué par des gestes, par la mimique et par des sons qui trahissaient des états d'âme comme la solitude, le désir ou la joie. Si les débuts du langage ont été tels — et c'est, nous semble-t-il, l'hypothèse la plus probable — ce n'est pas là dedans que consiste la différence entre l'homme et l'animal, au contraire: cette structure du langage n'est pas spécifiquement propre à l'homme, il l'a en commun avec les animaux. Le roucoulement du pigeon, le hennissement du cheval, l'aboiement assez varié du chien et tant d'autres conduites vocales ou musculaires semblent bien prouver que la vie animale possède une expressivité qui est aussi le partage de l'homme. Si donc il y a une séparation nette qui met l'homme à part dans le domaine vivant, la démarcation devra être cherchée au delà du domaine de l'expressivité que les hommes ont en commun avec les animaux supérieurs. Et ce ne sera pas le langage expressif, qui marquera le pasage au langage spécifiquement humain, mais le langage symbolique. Aussitôt que le langage prend une fonction objectivante, c'est preuve que l'esprit s'est liberé de sa passivité immédiate. Il est naturel de supposer que cette libération s'est appuyée sur l'expressivité et qu'elle en a utilisé certains éléments. Supposons que p. ex. l'impression produite par certains animaux dangereux se soit traduite par un son imitant le bruit que fait leur approche, l'imitation de ce bruit aura d'abord servi à prévenir les partenaires. Il aura eu ainsi une fonction instantanée et qui ne supporte pas de délai. Mais une fois le danger écarté les esprits se reposeront et ils se souviendront avec un mélange d'horreur et de jouissance les peines vécues dans la lutte. Revenu au calme, qui n'était qu'un intervalle entre deux batailles, l'esprit humain a pu transférer ses énergies de la lutte à l'activité paisible qui est leur souvenir. Il a pu, dans l'entr'acte, diriger les énergies accumulées vers un but plus élevé, puisque les circonstances en favorisaient un usage plus pacifique et plus intérieur: celui du souvenir, de la contemplation, de la fonction cognitive. Nous mêmes, qui savons par l'expérience de la plus désastreuse de toutes les guerres comment

les forces de l'esprit, habituées à s'exercer dans le domaine de la pure science, peuvent être rappelées à cet exercice presque oublié et insolite qui est la lutte pour l'existence, nous tous qui connaissons le détriment que souffrent les activités intellectuelles et spirituelles quand la guerre nous engage à rabaisser nos efforts à la défense contre la destruction, nous nous imaginons vivement, comment, à l'aube de l'espèce humaine, la pensée a pu se libérer, par des instants libres et par intervalles de durée incertaine du service qui elle devait rendre à la vie. Et c'est ainsi que l'esprit a pu reviser et comme créer de nouveau ce langage dont l'origine fut soudé à toutes les péripéties de la frêle existence que l'espèce humaine menait alors au milieu de la nature.

A mesure que la vie se stabilisait la fonction du langage a dû changer et l'élément émotionel a dû céder à l'intellectualisation. Les sons jusqu'alors avaient accompagné les actes des hommes par nécessité et là où l'excitation les faisait sortir comme involontairement. Dans la tension de l'action on n'y avait voué aucune attention si ce n'est ce minimum qui fut imposé par le souci d'être bien compris, en vue de l'effet désiré. Mais après la tempête, dans la sécurité et le loisir l'homme a commencé à voir autrement l'outil qui l'avait servi, il l'a envisagé non plus comme un outil, mais comme un objet. Et comme le primitif, dans ses loisirs, avait pris plaisir à embellir des outils avec d'autres outils et ainsi en faire dépasser le sens originaire, de la même façon ou toujours analoguement le langage a dû s'ordonner et se systématiser sous le tranquille regard rétrospectif de la réflexion.

S'il en est ainsi, et nous devons le supposer quand nous utilisons les données concrètes pour reconstruire avec prudence la préhistoire, l'hominisation, c. à d. la rupture essentielle avec la série des ancêtres animaux se manifesterait dans ce dépassement du stade expressif dont la conduite animale ne nous montre aucune trace et qui par conséquent est spécifique à l'homme. Les animaux réagissent bien par des signaux à des perceptions. Détacher le signal de sa fonction pragmatique et vitale, le prendre non plus pour l'indication d'une conduite à suivre, mais pour le symbole d'un objet à considérer, à contempler, à connaître, voilà le chemin dans lequel il s'est engagé seul et en laissant derrière lui ses ancêtres animaux à une distance qui devait rapidement se multiplier une fois que le principe symbo-

lisant avait pris son essor. Notre hypothèse a l'avantage que la création du langage n'incombe pas entièrement à l'homme, elle nous permet de retracer ses origines jusque dans la série animale. En même temps elle semble permettre de préciser en quoi consiste le passage à l'humanité. Ce passage serait une création se manifestant par une utilisation, à savoir la modification du signal en symbole, de la réaction en réflexion.

La réflexion qui stabilise le langage en l'interêt de la connaissance serait donc une déflection de la fonction purement pragmatique du langage. Elle est inspirée par un souci qui ne caractérise pas l'emploi du langage dans le stade originaire: celui de la conséquence, de la régularité et de l'universalité. Plusieurs passages de la protohistoire vers l'histoire s'éclaircissent sous le jour de cette hypothèse. On a souvent remarqué que dans le système verbal les formes les plus simples, ce sont l'impératif et l'infinitif, le dernier faisant souvent fonction d'impératif. RÉVÉSZ dans son précieux livre sur l'origine du langage (1946) a insisté sur ce fait pour appuyer sa thèse que tout le système verbal aurait dans l'impératif sa forme la plus archaique et comme sa source.

Cette thèse semble mériter toute notre attention. L'impératif en effet, de toutes les formes du verbe est celle dont le sens est le plus instantané et dont l'emploi dépend surtout des nécessités du moment. L'impératif a son sens dans le moment, l'exécution de l'ordre qu'il implique peut ne pas laisser de souvenir. C'est un jaillissement de la parole humaine, comme le cri d'angoisse d'une mère qui voit son enfant en danger et qui en même temps et indivisiblement extériorise sa frayeur et tache de le sauver. Ce cri, il est vrai, n'est pas encore le mot articulé. Mais il le prépare et si le cri de frayeur n'est pas un mot, le mot à son tour redescend facilement à la forme d'une exclamation. L'impératif est donc dans le système verbal l'élément le plus actif et instantané. On peut dresser un animal à une conduite qui correspond à l'exécution d'un ordre. C'est dire que l'impératif appartient au domaine linguistique commun de l'homme et de l'animal, comme c'est le cas pour l'expressivité affective. Et l'humanité commencerait au delà de l'impératif, au moment où, par une tranquillisation correspondante au loisir qui suit le travail accompli, l'esprit se replie sur le passé et le considère dans la forme du souvenir. C'est devant le regard de

la réflexion que l'impératif se modifie, et qu'il abandonne son caractère fugitif et tendu. C'est ainsi que de l'enveloppe de l'instantanéité il sort quelque chose de durable et tranquille: la forme indicative, qui est la forme de la connaissance. L'indicatif, pour l'esprit humain, est tout près de l'impératif, il y est impliqué. Comprendre un impératif, pour nous autres, c'est comprendre en même temps l'indicatif qui y est impliqué. En est ce de même pour l'animal?

Mon chien à qui je dis: „couche-toi" et il se couche, lui aussi a-t-il le sens de ce que c'est que se coucher puisqu'il réagit à l'ordre „couche-toi"? Sûrement non. Le sens de l'indicatif est humain et l'animal est un être qui réagit à l'impératif sans compréhension de l'indicatif correspondant. Ceci serait donc spécifiquement humain: la capacité d'arrêter le dynamisme vital des réactions et d'y intercaler, par le souvenir, la pure constatation. C'est par l'indicatif que l'ambiance vitale dont l'organisme est le centre se modifie en un monde, objet du désir de connaître et devant lequel le sujet humain s'incline et s'efface. L'indicatif est en effet la forme du souvenir qui transforme le turbulent vécu en la sérénité du passé souvenu. De l'objectivité du récit à celle de la science le chemin n'est pas long. Le langage des cris, des ordres, des menaces et des imprécations cède à celui de la constatation pure. Et cela n'est pas le cas seulement pour les choses du passé. La civilisation introduit partout la discipline de la constatation. Elle nous entraîne à l'appliquer p. ex. envers le médecin qui nous interroge sur la nature de nos souffrances, envers le psychiatre qui nous invite à dire en quoi consiste notre malaise, envers la chronologie précise d'un décès qui nous met en deuil ou d'une naissance qui nous remplit de joie. La constatation exacte, exprimée par l'indicatif, constatation qui défie nos sentiments fugitifs et égocentriques, pourtant si sobre, si vraie, quelle magnifique conquête de l'esprit humain dans la souveraine vérité, quel sujet de méditation pour les philosophes! N'est ce pas l'être lui-même dont la simple facticité est tout entièrement mystère, qui se révèle à nous dans la constatation? Et n'est ce pas l'indicatif du verbe qui nous rend capables de l'exprimer?

Mais revenons à nos problèmes. Nous avons reconnu avec Révész dans l'impératif le prototype du verbe et nous avons esquissé la transition de l'impératif à l'intellectualisation, dont

l'indicatif est la manifestation. Le verbe est bien la catégorie centrale du langage. Il accompagne l'activité humaine, qui est à son tour la forme essentielle de l'existence. Le va-et-vient continuel du réel impose à l'homme une activité pour son maintien. La lutte pour l'existence, le vouloir perseverare in suo esse de Spinoza, est à la base de tout. Le verbe, dans la forme de l'impératif, exprime la nécessité de l'action. En serait-il de même pour les autres catégories de mots? Est- ce que pour le substantif p. ex. on pourrait chercher également une forme prototype, qui présenterait le substantif avant que celui-ci soit devenu le soutien d'une réprésentation objective? Il y a, en effet, le vocatif qui exprime l'appel. Si le vocatif, qui souvent n'a pas de désinence et coincide avec le thème sans flexion pouvait être considéré comme le reliquat d'une série de formes affectives du nom, p. ex. le plaintif, le désidératif, le maledictif, le délectatif et d'autres, ne serions nous pas amenés par là à retracer une période du langage qui correspondait à une prédominance des émotions comme le notre correspond à une prédominance de l'intelligence?

Il est evident que tâchant de pénétrer dans l'obscurité de la préhistoire nous nous mouvons dans les hypothèses. La seule méthode qui vaille ici c'est l'extrapolation vers en arrière. Mais si la solidité en est recommandée par des applications contrôlables, il semble que l'idée d'un stade préhistorique non encore intellectualisé se justifie.

En effet, une fois que nous supposons que la rupture entre l'animal et l'homme s'est produite lors de l'objectivation du réel par le moyen du langage, nous voyons ce procédé d'objectivation se répéter plus d'une fois dans l'histoire de l'humanité et surtout dans les milieus de civilisation supérieure. La maîtrise rationelle qui a commencé par le langage comme résidu intellectualisé de l'expression émotive a dû être pendant longtemps identique au langage même. Le langage a été pendant une longue période la seule connaissance de la nature que l'homme possédait. Le stade des réactions instantanées a du être suivi par celui de la fixation, l'impératif par l'indicatif, le vocatif par le nominatif. Aussitôt que le nominatif a prévalu et que les objets ont cessé d'être des objets de réaction seulement pour devenir des objets de connaissance, la réflexion a pris les mots pour les véritables expressions de la nature des choses. Si dans le stade

affectif et instantané le langage fut un instrument auquel on faisait très peu attention, par opposition le stade intellectuel a commencé par une sorte de magie du langage, qui s'est prolongée jusque dans l'idéalisme platonicien et l'ontologie qui lui a succédé. Mais l'identification du langage avec la connaissance, qui est comme l'antithèse par rapport à la thèse originaire, n'a pas pu se maintenir en face du progrès de la science. Nous assistons au spectacle saisissant de la libération de l'esprit scientifique par rapport aux idéologies linguistiques dès le siècle de Platon. Le langage qui a magnifiquement servi à l'intellectualisation de l'esprit par rapport au stade précédent, à un certain moment de l'évolution humaine devient une entrave et se brise sous la poussée de l'esprit scientifique. Nous voyons les mathématiques se libérer des signes trop qualitatifs et embarrassants et les remplacer par des symboles adaptés. Nous voyons, d'autre part, que l'objectivation qui a produit le langage, ne s'arrête pas là mais qu'elle monte un second degré en objectivant son propre produit: c'est la science grammaticale qui naît ainsi. Le langage — et c'est assez curieux — fournit lui-même les instruments de sa propre objectivation, de façon que parlions en termes linguistiques des autres termes.

L'esprit humain se rend compte non seulement de la réalité qui l'environne, mais aussi de l'image qu'il se fait de cette réalité. Son pouvoir d'objectivation ne se limite donc pas à ses productions. La réflexion saisit d'abord le monde, puis son image du monde et finalement même l'image de cette image. Dans la mesure où s'accomplissent les objectivations successives, l'esprit se voit obligé de puiser dans son propre fonds pour trouver de nouveaux symboles objectivants. Les objectivations après celles du second étage deviennent de plus en plus pénibles et ardues. Mais d'entrevoir leur possibilité même est une attitude spécifiquement humaine. Imaginons un chien qui s'appelle Prince. Il „sait" son nom en ce sens qu'il accourt quand on l'appelle par son „nom". C'est qu'il réagit au vocatif et qu'il comprend ce vocatif. Mais il ne comprend pas si on lui dit: Toi, mon chien, tu t'appelles Prince, n'est ce pas? Et il ne dira jamais, en aboyant, bien entendu: Moi, je suis un chien, qui s'appelle Prince, ou tout simplement: je suis Prince.

Pour pénétrer l'origine du langage nous avons le choix entre une pensée hardie et pourtant consciente de sa faiblesse et un scepticisme résigné. Si nous choisissons pour le premier terme

de ce dilemme, nous devons nous engager dans la voie de la reconstruction et nous le ferons seulement en nous appuyant autant que possible sur les données de l'histoire. L'évolution du langage connue par l'histoire remonte à peine à cinq mille ans, ce qui est une très brève période dans l'évolution de l'espèce humaine. Mais elle est pleine de grands événements. Pour ce qui est du langage, dans cette brève période de quelques milliers d'années, nous assistons à une modification profonde et qui paraît plus ou moins analogue à la modification de la pensée ontologique en pensée rationelle. C'est la décomposition du langage-connaissance en langage symbolique ou fonctionnel. Par langage-connaissance j'entends un groupe de phénomènes, qui sont en train de disparaître dans nos langues modernes, comme p. ex. le genre grammatical, le masculin, le féminin, le neutre. Nous les respectons plus ou moins, puisque la tradition est tenace et lente, mais dans certaines langues, le néerlandais p. ex., la distinction du genre masculin et féminin est maintenue ou était maintenue péniblement par l'enseignement.

Récemment, par décret du Ministère de l'Education nationale, nous avons été autorisés à faire dans la langue écrite ce que nous faisions depuis longtemps dans la langue parlée, de laisser tomber les différences du masculin et du féminin grammaticaux. Cette distinction a eu autrefois un sens. Nous la retrouvons dans plusieurs langues primitives, où à côté du masculin et du féminin d'autres distinctions grammaticales ont existé. Ce sont des classifications et les classifications sont des produits de connaissance primitive. Or, quelle est la raison possible, éventuelle pour laquelle l'expression de cette connaissance classificatrice est en voie de disparition? Il me semble que c'est puisque la connaissance emmagasinée dans le langage ne vaut plus à côté de la connaissance conquise par l'expérience scientifique. La justice au féminin, pourquoi? Est ce que la femme est plus juste que l'homme? On peut en douter. Mais l'amour, masculin, pourquoi? Est ce l'apanage de l'homme plutôt que de la femme? On pourrait en douter également. Cette classification qui a eu un sens autrefois, puisqu'elle correspondait à un état de connaissance, l'a perdu totalement, elle est devenue ce qu'il y a de plus traditionnel et bizarre. Aussi la connaissance-résidu que continue à exprimer le langage est du poids mort et la vie tend à s'en libérer. Pareille libération s'est accompli dans l'anglais moderne. Cette langue paraît sous plusieurs rapports la plus

avancée de toutes, si l'on envisage que l'évolution du langage qui une fois a consisté en l'intellectualisation de l'affectivité consiste maintenant en le divorce entre langage et connaissance. Le paradoxe que nous osons poser est celui-ci qu'à mesure que le langage perd en expressivité cognitive il gagne en effectivité symbolique. C'est que la connaissance est devenue autonome, c'est que pour elle, tout ce qu'on peut déduire du langage par rapport à la réalité n'est qu'une apparence de connaissance ou une connaissance qui ne l'est plus, puisque le divorce entre connaissance et langage est sanctionné par l'histoire même de la civilisation. S'il en est ainsi et si nous devons séparer les prétentions cognitives du langage pour en retenir le symbolisme au service de l'entente humaine, il faudra dorénavant nous méfier de certaines habitudes d'interroger le langage afin d'en obtenir des informations de connaissance. Il faudra ne plus s'adonner à cette magie des sexes grammaticaux et méditer la sagesse profonde qui fait la justice féminine et l'amour masculin, il faudra surtout cesser de s'attendre à ce que l'étymologie des mots nous renseigne sur leur véritable sens et même il faudra comprendre que la soi-disante analyse phénoménologique est toujours menacée du danger de s'entendre sur le sens commun d'un mot, ce qui est une question de sémantique et non pas de connaissance.

En jetant un regard rétrospectif sur l'évolution du langage, nous disions que le premier stade hypothétique a été caractérisé par la prédominance du sentiment et de la volonté, et qu'elle a été suivie par une phase d'intellectualisation, qu'une vie devenue sédentaire et exempte du souci prépondérant de la satisfaction des besoins vitaux, avait rendue possible. Dans cette phase, le langage, tout en étant le résidu d'un outillage expressif et pratique, représentait en même temps la connaissance emmagasinée par l'expérience du groupe. L'effort cognitif en cette phase s'appuyait plus sur le langage que sur le réel. Le stade où la civilisation est parvenue serait caractérisé par une libération de la connaissance par rapport au langage, qui, s'il ne dirige plus l'esprit cognitif, ne perd rien de sa valeur d'expressivité et de suggestivité esthétique. La recherche de l'origine du langage ne devient féconde qu'en se détournant de l'origine supposée pour prendre comme point de départ l'évolution de la civilisation ultérieure afin de s'approcher, par ce détour et rétrospectivement, du grand mystère de l'hominisation.

VISUAL FIGURES APPARENTLY INCOMPATIBLE WITH GEOMETRY

BY

EDGAR RUBIN (Kjöbenhavn)

The visual world contains a great amount which does not appear to have anything to do directly with geometry, such as colour, meaning, or, for instance, the visual experience that coffee has been spilt on a tablecloth. There is, however, a very large field where there seems to be some sort of relation.

In this paper only one special problem regarding this connection will be taken up with the help of some simple figures.

§ 1. If an uninitiated person just looks at an ordinary "geometrical optical illusion", for instance figure 1, the so-called "Müller-Lyer (or Brentano) Illusion", there is for him nothing peculiar with what he *sees*. He is first astonished when you ask him to make some sort of rough measurements of the lines *a* and *b*. He had thought, or had had the prejudice, that there was a correspondence between what he saw and the physical knowledge that the measurements would give — which there is not. We ordinarily believe that, at least with near objects, the visual magnitude is solely dependent upon the physical, the "real" magnitude of the object; or rather, we believe that what we see *is* the real or physical magnitude. It is a matter of common *physical* experience that other lines — for instance the Müller-Lyer device (the oblique lines in figure 1) — have no influence on the *physical* length of *a* and *b*. By comparison of the visual and the physical, we learn with the help of figure 1 that the Müller-Lyer device *has* influence upon the *visual* length.

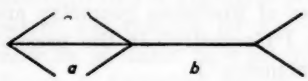

Fig. 1

This difference between the behaviour of the physical and the visual line is one among many facts from which we learn that the visual and the physical figures are different things. In daily

life they are usually confused, and it demands a certain amount of training to keep them apart [1]). We cannot here enter into the very complicated relationship between them, we can only emphasise that it is of the greatest importance for our problem to keep them separate nearly in the same way as visual colour must be kept apart from the electromagnetic waves.

Only one more argument for keeping them separate will be mentioned. There is a time lag between the physical stimulus from the figure and the visual experience, namely, the time taken by the elaborative processes in the nervous system.

In the following we ought to have used different terms for the physical and the visual objects, but in order not to make the text too complicated, we have often used the same, and thus the difference must be gathered from the context.

§ 2. By using some figures in which are incorporated illusion devices we will demonstrate something which seems to be both astonishing and paradoxical, *not only when you compare that which you see with measurements, but also when you keep strictly to the figure as a visual experience.* It seems that it can be demonstrated that the rules of Euclidian geometry are not valid for these visual figures. ("Geometry" in the present paper always means "Euclidian geometry".)

§ 3. Figure 2 is the well-known "Sander Illusion". We see that the line *AB* is shorter than *AC*. Crude measurements show that the two lines are physically alike. We will now use the principle of the "Sander Illusion" to construct a more complicated figure. This is figure 3, which is composed of two similar "Sander" figures *ABEF* and *BCDE*. If we just look at this figure as a whole, we can describe a part of what we immediately see as three horizontal, parallel lines — *AF*, *BE* and *CD* — where the distances between *BE* and *CD* and between *BE* and *AF* are equal. According to geometry, the two oblique straight

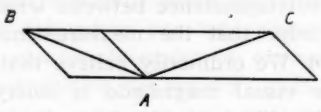

Fig 2

[1]) Compare the first pages of "Synsoplevede Figurer", Copenhagen, 1915, or „Visuell wahrgenommene Figuren", Copenhagen, 1921, and „Zur Psychophysik der Geradheit", Z.f. Psych., Bd. 90, 1922, reprinted in „Experimenta Psychologica", Copenhagen, 1949.

lines *AD* and *CF* passing through the point *O* should both be bisected by *BE*; this is easily demonstrated. Our immediate visual experience of the seen figure, however, is that the parts are unequal, *OA* and *OC* being shorter than *OD* and *OF*. We have here a case where three visually equidistant parallel lines do not visually bisect oblique lines. This easily demonstrated geometrical property holds for the physical lines as well as for what we immediately see when we have to do with three parallel, equidistant lines without the illusion devices. — This can be verified by looking at figure 4.

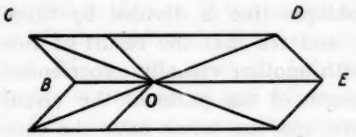

Fig. 3

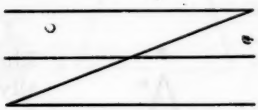
Fig. 4

§ 4. The incompatibility with geometry which has been demonstrated is our main point. In order to arrive at a second which has interest for the discussion of our main point, try now to go back to what you first saw when you looked at figure 3. Probably you will concede that at first everything looked quite in order; the contradiction only appeared to you with the formulation, or with the beginning of the formulation, of what you saw in linguistic form. We are here dealing with something general in the domain of perception. The eventual contradiction is not in the perception as such, but first makes its appearance along with the more or less explicit formulation in linguistic form (judgement form). This observation is contrary to what is maintained by those adhering to the school of Hegel, e.g., Bradley, when they speak about contradictions in the things themselves. I have never been able to understand this way of speaking. That is probably one of the reasons why I have never been able to appreciate Hegelian doctrines.

Let us try to translate into an explicit form that which happens when we look at figure 3, keeping in mind "traduttore–traditore". We may say that we arrive at the contradiction by starting with a visual *fact* which we denote "three lines seen to be parallel and equidistant". To this name (consisting of all

the terms between the inverted commas) we give the same connotation as in geometry [2]).

We then *deduce* how "an oblique line is divided by three equidistant and parallel lines" and *see* that the result of this deduction is in contradiction with another visually experienced fact which we denote "the length of the parts of the visual oblique lines *AD* and *CF*". Here, too, the terms have the connotation belonging to them in geometry. As what really happens takes place so quickly and easily, and as we are hardly conscious of it, we would, if asked, say that we saw the contradiction immediately.

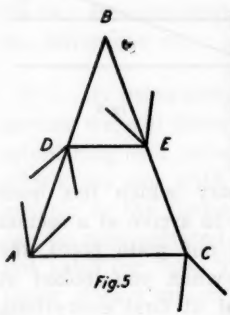

Fig.5

§ 5. On the basis of the "Müller–Lyer Illusion" we can construct another visually non-Euclidian figure. If you look at figure 5 you have the immediate visual experience of an isosceles triangle *ABC* (*AB* is equal in length to *BC* and the angles *BAC* and *ACB* are alike). You also have the immediate experience that *AD* is much shorter than *EC*. The curious, and for us important thing, is that in addition you have the immediate visual experience that *DE* is parallel to *AC*. It is quite apparent, without any detailed demonstration, that there here seems to be a contradiction with

[2]) Words (names) are many-sided things. We will just look at two sides to make clear what we mean by "denote" and "connote" — terms which, with some modifications, are taken from John Stuart Mill. If you look at words (names) serving as a sign for some object without regard to what they eventually say about the properties of the object, we call this marking, identifying or pointing function, "denotation", and use the verb "to denote". If you look at what the words mean to you concerning the properties and relationships of the object and the knowledge of which they are a vehicle apart from that which is inherent in the pointing, we speak about "connotation" and use the verb "to connote". The words "my pencil" denote or point to a certain object and they connote (for me) certain properties and relationships belonging to this object.

The same words may denote the same object for different individuals, but, probably due to the different backgrounds of these individuals, they have a different connotation. In the same way the connotation for one individual can be different in various situations although the denotation is the same.

geometry. In geometry it can be demonstrated: "If $<BAC$ and $<ACB$ are equal and if AD and EC are not equal, then DE is not parallel to AC; or, if the angles are equal, and if DE is parallel to AC, then AD and CE are equal; or, if DE and AC are parallel, and AD is not equal to CE, then the angles are not equal and AB is not equal to CB". None of these propositions hold for what we immediately see.

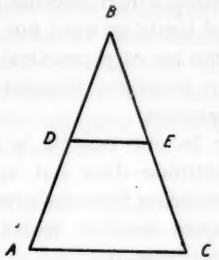

Fig. 6

In this case, as in the case with the "Sander Illusion", the experienced contradiction arises with the formulation.

If you look at figure 6, which is without the Müller–Lyer device, the figure regarded as a visual experience is in order from a geometrical point of view.

§ 6. There is a third point of interest for us which can be attacked with the help of figure 5. Try to look at the figure, and concentrate on the fact that AC and DE are parallel. You have, so to speak, to centre your whole way of looking at the figure upon the fact that AC is parallel to DE. It is very natural to do this when you are introduced to the problems of the figure. After a time the visually experienced difference between the lengths of AD and CE will become less and less, without quite disappearing. At the same time as the perceived difference diminishes, the still remaining difference looses, to a certain degree, the character of reality.

That with which we are dealing in this case is, that on the basis of the same outer stimulus situation, but with another mental set than the original, an altered elaborative process takes place in the psycho-physiological system. It is a well-known fact that an analytical mental set alters the elaborative processes in such a way that the "Müller–Lyer Illusion" and many other "illusions" become less pronounced.

In addition, the centering on the fact that AC and DE are parallel, and on all the knowledge which this ordinarily involves, makes the visual difference between AD and CE which is incompatible with this knowledge, seem less real. It often happens that we get this diminished feeling of reality when something does not agree with our knowledge of what is

(believed to be) general experience or a ruling prejudice. Often a certain attitude accompanies this feeling of unreality. The effects of this attitude can be described as the putting aside of a "disagreeable" fact. You treat it in an offhand way as something which does not really exist, something which is not worthy of being or need not be taken seriously. This attitude certainly can be very practical in general life, but when you are engaged in scientific research work it can have very unhappy consequences.

In our case it is clear that this putting-aside-as-negligible attitude does not agree with an attitude of wholeheartedly entering into our problem. You therefore have to get over it in some way — which from my own experience I know can be difficult.

§ 7. It may be opportune to say that it is possible to deal with a figure in such a way that at one moment you are interested in one feature of it, and at another in a different feature. You can, for instance, at one moment be interested in the lengths of *AD* and *CE* (figure 5) and at another in whether or not *DE* is parallel to *AC*. This successive domination could be so complete that when you experienced something regarding the lengths of *AD* and *CE*, you did not, or could not, experience anything regarding the directions of *DE* and *AC*. In this case you might be inclined to use this to try to evade our problem in the putting-aside-as-negligible attitude. This domination, however, is not, or need not, be complete. You can easily ascertain that although you maybe are most interested in one thing, you can have visual experiences regarding the other at the same time. In our arguments we will keep to such cases.

§ 8. It is very easy to construct many other figures where there is apparent incompatibility with geometry; for instance, figure 7, where the "horizontal" lines *AB* and *CD* are physically equal. Although you see the "horizontal" line *AB* as longer than and parallel to *CD*, you also see that the "vertical" lines *AC* and *BD* are parallel and are perpendicular to both *AB* and *CD*. The first part of the experience

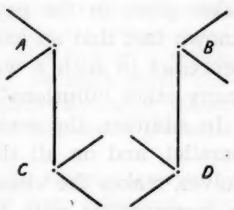

Fig. 7

($AB > CD$ and $AB \neq CD$) would geometrically imply that AC and BD converge toward a point below CD, but you experience AC and BD as parallel to each other. The second part (AB and CD perpendicular to both BD and AC) geometrically implies that AB and CD are equal, but you experience them as unequal. Both ways of looking at the situation give, as in our other cases, a contradiction between one visually experienced fact and an extremely simple geometrical deduction from another. According to my own experience, the contradiction, although quite marked, is not so strongly felt here as with figures 3 and 5.

§ 9. Many years ago I began to be interested in this figure and others analogous to it. I had a very vague, and not very explicitly formulated idea, that the explanation of the apparent incompatibility was to be found in a certain doubleness. I thought that the physical stimuli AB and CD each occasioned two processes, one resulting in the apparent length of AB (and of CD), and another probably remaining purely physiological. I had an idea that the first process was much more dependent on the mental set of the subject and on the whole stimulus situation — especially the illusion devices — than the second, which I thought was more dependant on the main stimulus alone. Therefore this second process was, so to speak, geometrically more reliable and in a sense also more "objective". I thought that the physiological processes, and not those of the first type, might be among the conditions leading to other visual experiences regarding the two lines — for instance, the experiences regarding the relative positions of the pairs of points A & C and B & D. I also thought that the feeling of unreality — of which I have given another explanation above — was a result of the discrepancy between the two processes.

§ 10. My colleague, Professor E. Tranekjær Rasmussen, has also worked with figure 7. He has put forward that the visual experience of magnitude as a rule is more dependent on the whole stimulus situation and the mental set of the subject than the experience of "direction", especially the experience of parallelism. He finds that certain other experiences from an unpublished study about visual parallelism to a certain degree corroborate this point of view.

He has further put forward figure 8 as being something in

the nature of a proof. Here the relationship between the points *A & C* and between *B & D* is much less pronounced from the stimulus point of view, and therefore probably less resistant. He has found by means of elaborate and systematic experiments that *AC* and *BD* are seen as converging towards a point beneath *CD* at some point on a vertical line from the centre of *CD*. This point is further away than you would expect if you make some sort of geometrical constructions with the apparent lengths of *AB* and *CD* as starting point. Quantatively, therefore, geometry does not hold. There is only a certain approximation to it. If you try to verify his results with the help of figure 7, maybe everybody will not agree with him. Personally I have some difficulty, but there are many sources of possible error in the crude experimental work which can be carried out with figure 7. Besides this, there can, to a certain extent, be personal differences.

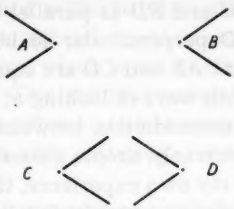

Fig. 8

I may mention here that I have had great pleasure and very much help from discussions with Professor Tranekjær Rasmussen concerning the topics of this paper.

§ 11. The adherents of "wholes" maintain something like the idea that a whole or gestalt is characterised by the fact that all parts in a whole are interrelated in such a way that everything alters — or, more cautiously, tends to be altered — if one part is altered or added. If you now, for instance, compare our isosceles triangle with and without the illusion producing device (figures 5 and 6), our main fact can be stated thus: although the adding of the device visually alters *AD* and *EC*, it has no effect on the visual direction of *DE* or of *AC*. This is not quite what you would expect if you believed in the general statement regarding wholes. cf. § 20 and 21.

§ 12. Let us in a general way look at geometry in relation to our main point. If you have a geometrical figure, there are geometrical facts concerning this figure which are mathematically related in such a way that if one is altered, the others are necessarily altered. For instance, you cannot alter just one side

of a triangle, you must necessarily also alter at least two of the angles. If you, in figure 6, geometrically alter the position of the point *D*, the direction of the line *DE* is unavoidably altered in relation to the direction of the line *AC* [3]).

In a figure regarded as a visual experience, however, facts corresponding to such interrelated geometrical facts can occasionally be relatively independent of each other so that one can be altered without alteration to the other. For example, from a visual point of view the relative lengths of *AD* and *CE* in figure 5 can be altered with the help of the illusion device without influencing the direction of *DE*.

The necessary relationship between geometrical facts is the main content of the propositions of geometry.

§ 13. On the other hand, you can make some alterations, e.g., additions, to a figure which, geometrically, leave all the former mathematical facts and relationships regarding the figure unaltered. Such alterations can, however, have marked influence on the corresponding facts belonging to the visual figure. An instance is if you have a straight line with a point in the middle (figure 9*a*). If you add (a part of) the Müller–Lyer device

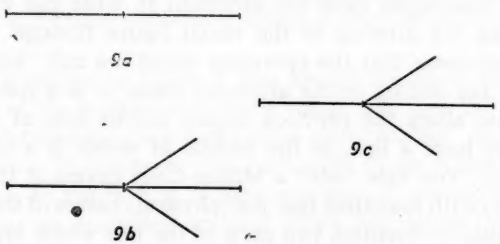

Fig.9

(figure 9*b*), this does not alter the geometrical relationship between the length of the two halves of the line. It does, however, alter the visual relationship between them.

In all the *proofs* of geometry where auxiliary lines are intro-

[3]) Where we above have used the word "altering" in connection with geometry, it may be more correct to say: Two figures having certain elements alike and a single element different, have also other elements which differ.

duced, you use as a basis that the lines added to the figure do not alter the former facts and relationships.

In textbooks of geometry you do not, as far as I am aware, find any axioms which speak about this. It seems to belong to the general background which is not really analysed. In this background it sometimes can be hidden in the form of the introduction of "ideal rigid objects". Another idea which can serve this purpose seems, if I am not mistaken, to lie behind what is explicitly stated in David Hilbert's classical "Grundlagen der Geometrie" (7th ed., 1930). It seems to me that Hilbert has the idea that when a geometry is defined by its axioms, the collected elements of this geometry — all the points and lines and surfaces — *exist* in some very abstract and very formal way. This involves that nothing happens in that world, no alterations are possible; the auxiliary lines as all other elements were there in this extremely formal way before we used them. When Hilbert, in order to make his text more easily readably, uses words which seem to imply alterations, they stand, as a matter of fact, for well defined ideas which do not imply alterations

§ 14. You might raise the objection to what has been said concerning the altering of the visual figure through addition of new elements that the operation which we call "adding" — which is not adding in the algebraic sense — is a specific one which also alters the previous figure. Let us look at the case where we have a line, in the middle of which is a point (*P*) (figure 9*a*). You now "add" a Müller–Lyer device at this point (figure 9*b*) with the effect that the (physical) halves of the line no longer visually condition two parts of the line which are visually equal. It seems defensible to say that the point *P* which you had at the beginning is visually identical to the one you have when the Müller–Lyer device has been added. You definitely *see* that you are dealing with the same point during the process of drawing the device. The following, however, could also be said, although it sounds a little strange before you have thought it over: *P* is *defined* as a point at the middle of the visual line. The procedure by which you "add" the Müller–Lyer device should be such that the converging point of the device is seen to be at the middle of the line after the device has been added.

It would be something like figure 9c. With regard to the outer physical stimulus this point lies physically to the right of the original point. According to the first way of looking at the matter, this is a new point on the line. According to the second way, it is the same due to the definition of *P*.

Let us alter the situation a little. Instead of going through the process of "adding", have the three figures (9*a*, *b* & *c*) laid before you. Let us suppose that you want to keep strictly to the visual figures and regard *P* as the visual middle point of 9*a*. Here, perhaps, you will feel it more reasonable to say that 9c is the "same" figure as 9*a* with the Müller–Lyer device "added". I have made some experiments, the results of which are quite in accordance with that which has been put forward here.

I won't enter into a deeper discussion of these two, or other possible ways, of looking at our problem. I merely wanted the reader to have the impression that we are not dealing here with something which is plain sailing. Although you very often feel perfectly certain when, in a case before you, you have to decide whether or not you are dealing with identity or not, you would have great difficulty if you had to give good reasons for your decision. You would then find that the foundation of what you regard as identity is not something uniform, simple and clear, but on the contrary something which is very variable and complicated.

If, with regard to figure 9, you accept the view where you define *P* as the middle point of the visual line, then the previous visual relationships are still the same after "adding" the Müller–Lyer device. If, however, you start with figure 6 and add the devices as in figure 5, it is here apparent if you try that you cannot use an analogical method which will allow the former relationships to remain unchanged.

§ 15. We have now looked at pure geometrical and visual figures; let us turn to a physical figure, for example an isosceles triangle made of metal. If you here, through ordinary physical agencies (for instance, warming and cooling) alter the lengths of *AD* and *CE*, then the whole configuration will alter through the working of physical forces. This will happen in such a way that, contrary to what occasionally can happen with visual figures, the new configuration also will be in accordance with

geometry. This also holds for the new configuration which will arise if we break up the piece of metal into small pieces. It is, however, difficult to find a clear answer to the question "What is to be said if, for instance, the triangle is disolved in acid?"

§ 16. Up to now we have treated the visual figures as objects before us with which we were dealing. Let us now look at the situation, for instance, with the isosceles triangles in figures 5 and 6 as a matter of *processes of perception.* We suppose we have before us the physical triangle without the Müller–Lyer device (figure 5). This physical stimulus occasions processes in the nervous system which lead to the psychological experience of the visual triangle. Some believe that all the nervous processes occasioned by the stimulus, from the processes in the retina to the very highest in the brain, immediately connected with the seen triangle, are of a physical nature, and that the last mentioned processes are isomorphic with the triangle as experienced visually. "Physical" here means, if it is to be clear, "Something belonging to physics as taught at present". "Isomorphic" means that there is a (formal) similarity in structure. If we now, in passing from figure 6 to figure 5, introduce the Müller–Lyer device, this will alter the picture on the retina and the nervous processes including the very highest in the brain. If these resulting brain processes as postulated are of a physical nature, they must, after that which physics teaches us, be in accordance with geometry. The figure which we see, however, is *not* in accordance with geometry. It is difficult to see in a case like this how adherents of isomorphism can find their way out.

Pointing out this difficulty for isomorphism does not mean that every form of isomorphism is shown to be impossible; maybe you can think out a very sophisticated sort of isomorphism which can help you. Neither does it mean, I am sorry to say, that I can positively offer a better way out regarding the relationship between the highest brain processes and the psychological fact of the figure which we see.

§ 17. Let us try to go a little deeper into our problem. For this purpose we will look at that form of geometry which appears in Hilbert's "Grundlegung der Geometrie" (pages 1–32). I will call it "abstract" or "generalised geometry". Hilbert begin with introducing some arbitrarily selected words: "points",

"straight lines" and "planes". He does not say anything at all about what these arbitrary words denote, but in the axioms some very abstract and formal relationships between whatever they denote are put forward very precisely. For instance, "to two points corresponds a straight line, which belongs together with each of them". Hilbert could just as well have selected other words than those chosen, such as "fup", "dup", "lup" or whatever you may think of. The axioms together with logic form a system. From this system you can deduce in a logical way all propositions belonging to abstract geometry.

If you now become acquainted with some objects for which the relationships about which the axioms speak are valid, you can arbitrarily let the selected words denote these objects. You can, for instance, if you have a field of numbers defined in a suitable way, let a pair of numbers (a and b) be denoted by the word "point" and let the relationship between three numbers (u:v:w) be denoted by "a straight line".

The above mentioned axiom concerning the straight line now means that what we here understand by two points, namely, two pairs of numbers, for instance: $\left(0, \div\frac{w}{v}\right)$ & $\left(\div\frac{w}{u}, 0\right)$ belong together with that which here is understood by a straight line, namely (u:v:w:) or $ux + vy + w = 0$.

All *propositions* belonging to abstract geometry are valid in this field of numbers if you let the arbitrarily chosen words denote the objects of this field. I will speak about "number geometry". All the propositions you find in this field are also inversely valid in abstract geometry, if you disregard the denotation of the words "point", etc., in this field. I say that they can be "transferred". My insufficient insight prohibits me from entering into how it is as regards the transferability of *proofs*.

This application of the absolutely abstract words to numbers is the first step which leads to what is often called the analytical geometry of Cartesius.

According to experience you can also use the words of abstract geometry to denote a sort of idealised physical objects, namely, those which in daily life, for instance in the schools, are understood by "points", "straight lines", etc. I then speak about "elementary geometry". You have here that all propo-

sitions from abstract geometry are valid in this field when you use the words to denote the chosen objects in this field. Inversely you can "transfer" the propositions from this field to abstract geometry.

I ought to have made a distinction between these idealised physical objects and real physical objects, also to have studied this distinction, but I cannot enter into this here. In a series of important papers, the late Professor Johs. Hjelmslev of Copenhagen University has gone into problems which are connected with this matter.

Either through passing from elementary geometry via abstract geometry to the geometry of numbers or more directly by connecting elementary geometry to number geometry as Cartesius probably has done, it can be demonstrated that every proposition valid in the one field is also valid in the other. When this has been done, you have that which is ordinarily understood by "analytical geometry".

Regarding "abstract geometry", which I look upon as systematically fundamental, it may be said that the historical evolution which has led to it has probably been such that it commenced with elementary geometry or something more primitive and then through abstractions, generalisation and afterthoughts arrived at "abstract geometry". It is this historical process which has led to the use of the words "point", "straight line", etc., in abstract geometry. This can, if you do not keep your thoughts sufficiently clear, make it difficult to achieve a clear understanding of the nature of abstract geometry; the words are not made sufficiently free from the special meaning they have in elementary geometry.

It lays near to hand, either through going via abstract geometry or through direct comparison with elementary geometry, to try to find (with cautious regard to the character of the material) a connection between geometry and some strictly visual objects, which it seems natural to denote as "visual points", "visual straight lines", etc.

I may perhaps here refer the reader to my demonstration of the existence of visual points without extension and visual lines without breadth. I can also mention my proof of the proposition that between two seen points we can see only one straight line. Here, "straight line" must be understood as

denoting a specific property pertaining to seen objects[4]).

There is certainly much relating to elementary geometry which you would think quite naturally applies to the field of visual objects; in this paper such applications have often been made. It is worth while mentioning in this connection that in daily life physical objects — for which it is supposed that elementary geometry holds — are not kept apart from visual objects (cf. § 1 above).

It is, however, important to be cautious — a certain reservation lies already in the words used above "with cautious regard to the character of the material". Regarding a special point where the relation does not hold, I can refer you to the above mentioned paper in Zeitschr. f. Psych. and Experimenta Psychologica. It is here demonstrated that what in geometry is understood by the straightest line (the geodetic line) on a surface other than a plane is something quite different from that which visually appears as the straightest line on such a surface.

Professor Tranekjær Rasmussen has worked with some very interesting cases of another nature where there is no agreement between some visual objects and elementary geometry. It really may be that the difficulties in his cases concern something deeper, so that even ordinary logic does not hold. It is to be hoped that he will very soon publish a paper dealing with this subject.

§ 18. In the present study it has been shown that if we apply geometry to some seen figures we meet with contradictions — not in the figures as such, but in our more or less formulated descriptions of them. In all these cases we have dealt with seen parallel lines.

A study of the situation thus encountered — empirical visually perceived figures where geometry is not valid — may be interesting from the point of view of geometry in order to elucidate further the fundamentals of this science. Such a study is certainly interesting from the points of view of philosophy and of psychology.

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[4]) Compare "Synsoplevede Figurer", page 180, or, "Visuell wahrgenommene Figuren", page 193 ff., and "Zur Psychophysik der Geradheit", Z.f. Psych., Bd. 90, p. 77, 1922, reprinted in "Experimenta Psychologica", page 93 f.

It seems that deeper reflections give as a result that that which we denoted with a name like "seen parallel lines" does not in all cases have all the properties which it must if it is to be defensible to take over the term "parallel lines" from geometry[5]).

It might be awkward on this account to discontinue speaking in this connection about "parallel lines" because it is an old custom to do it and most people probably have, more or less vaguely, something like this in their mind when they speak geometrically about parallel lines, and lastly, it has as yet caused no trouble to do so. The only thing is that from now on you have to be cautious and when necessary speak about visual parallelism in order to remember that the connotation occasionally can be or is another than that in geometry.

§ 19. The conclusion to which the preceding section has led, viz., that visual parallelism can not be considered as parallelism in its strictly geometrical sense, entails that the contradictions between geometry and the experiences with visually experienced figures are only apparent. They depend simply upon a mistake you are involuntarily apt to make. You presume that what can rightly be deduced from geometric parallelism in a geometrical world also applies to visual parallelism in a visual world. You confuse visual parallelism with geometrical parallelism.

To a certain degree this is to say the same as has already been pointed out in § 18. It was, indeed, just this contradiction which — combined with a closer consideration of geometry — led to us maintaining that visual parallelism, even though there is a certain relationship which makes confusion possible, is *not* a form of geometrical parallelism. It is nevertheless not because we wish to avoid contradiction that we have said that visual parallelism is not geometrical parallelism. We have put forward very definite, new experiences to support this assertion.

Even so, however, the claim appears to be a little quasi-formal and does not give complete satisfaction. In a case like this, we are not satisfied by only pointing out a single and isolated difference between two things which we formerly have not separated. You want to convert the one difference into a link in

[5]) cf. analogous suggestions regarding "seen length of a line" and "seen areas" in § 24 and 25 below.

a more extensive chain. You can also say that we do not feel satisfied by merely demonstrating that there is a difference; we also desire an "understanding" of on what this difference depends. This means, among other things, that one desires a closer, factual account of the character of visual parallelism, or, to be more accurate, of the laws which apply to it. The new facts regarding our figures would then no longer be something isolated, but be a feature of a more extensive context regarding visual parallelism. An essential contribution to such a doctrine will, I believe, appear when Professor E. Tranekjær Rasmussen's investigations referred to earlier are published.

§ 20. It is possible that the understanding or explanation which we seek lies not only in a closer investigation of visual parallelism but also, and perhaps even to an equal degree, in the difference between the strictly logical and necessary way in which the factors of a geometrical figure are connected and the quite different kind of looser connections which govern a visual figure. This looseness is a consequence of the fact that the presence of the visual figures from second to second is due to the momentary psychophysiological processes. Behind the visual figure therefore, lies a most highly complicated causal relationship, while with a geometrical figure only (or apparently only) logical mathematical deductive relationships exist.

§ 21. We will embark upon somewhat airy speculations where our knowledge is minimal. Behind the visual figure lies a most complicated network of psychophysiological processes. Let us, without thinking of changing anything of that which actually happens, consider various partly overlapping aspects which are abstracted from that network in such a way that to each of the aspects correspond the various relatively independent visually experienced factors in the seen figure concerned: length, direction, junctions, equal length, same direction, etc., etc., all as features of the figure. Let us stress that "aspect" in the following concerns the psychophysiological processes, and "factor" or "feature" are applied to the visual figure.

If we commence with figure 6, for example, there is such a complicated coordination or cooperation between the aspects that the experienced figure is to a certain degree in agreement with geometry. If we then turn to figure 5 as stimulus, there

then appear new aspects in the psychophysiological processes corresponding to the Müller–Lyer device; at the same time the other processes are modified to a greater or lesser degree. If the new visual figure should also be in agreement with geometry, then the aspect of the Müller–Lyer device should either have absolutely no effect upon other aspects, which it does, or it should modify a large number of the other aspects. This, however, does not take place to a sufficient degree. One can here talk about a certain "looseness" in the "coupling" between the various aspects. This looseness shows itself not only in the fact that there are aspects which remain unaffected by the Müller–Lyer aspect, but also in that the changes in certain aspects conditioned by the Müller–Lyer device have no effect upon other aspects. For instance, the Müller–Lyer device affects the aspects which correspond to the visually experienced lengths of *AD* and *CE*; however, neither the Müller–Lyer device or the changed aspects of *AD* and *CE* have any effect upon the aspects corresponding to the direction of *AC* and *DE*.

When we here have spoken about a certain looseness in the coupling between aspects, we could also have put forward a slightly different point of view (which perhaps, as long as our knowledge is minimal, is only a slightly different way of speaking) and have said that the one aspect is comparatively resistant to the other. This corresponds to Professor Tranekjær Rasmussen's point of view given in § 10.

As long as we are dealing with the matter as we have done here, we can only get hold of looseness in the couplings in those visual figures which are not in agreement with geometry. Perhaps this has a deeper significance. A geometrical figure is characterised by the deductively demonstrable rigid correlation between its factors. If a visual figure is in agreement with geometry there can, or must be imagined an intimate causal coupling of a very specific nature between the various aspects by which they are coordinated so that the visual figure looks as though the factors were logically and deductively connected with each other.

From these considerations one can understand that there must be a certain complicatedness in a figure which can be used for demonstrating disagreement with geometry. If there are not several factors, which, for example, there are not in a simple

Müller–Lyer figure (figure 1), there is no basis present for the looseness to make itself shown.

One can consider supporting these speculations about functional looseness by bringing forth that in our figure 5 where the Müller–Lyer device is used, there can, with a little goodwill, be sensorially experienced a certain looseness or fluttering characterising the visual figures. However, there needs to be a lot of goodwill in order to find something similar with the double Sander figure (figure 3).

§ 22. The considerations we have put forward can — even if they have no other value — lead to the question as to whether the effects of the Müller–Lyer device are actually as isolated as one might consider on the basis of what we have said earlier about figure 5.

It appears that this is not the case. Let us first look at the interesting part-figure in figure 5 consisting of the visual triangle *DBE*. It is seen in a very peculiar way — it appears as a visual isosceles triangle where the one side is seen as being longer than the other.

It will be better if we select another term so that we do not come to speak about an isosceles triangle which is not isosceles. The word "isosceles" in this connection has two meanings. Let us consider a number of various visual isosceles triangles. There is a common visual trait in them. That which is common to them has previously been denoted by the word "isosceles". We will instead denote this trait or quality by "*i*". We could, for that matter, speak in Ehrenfels' way and talk about the Gestalt quality "*i*". Our part figure shows now that this "*i*" trait can be found in visual triangles other than those which are specifically experienced as being isosceles. We can go further. We can construct a triangle *DBE* (figure 10) where *DB* is geometrically shorter than *EB* but where the adding of the Müller–Lyer device involves that *DB* and *EB* are visually about equal. The small mark on *EB* shows how long it should be if it were to have the same geometrical length as *DB*. It will be seen that the sides are visually equal in length, when

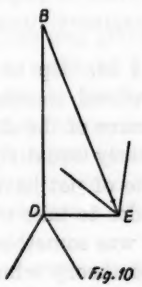

Fig. 10

you keep strictly to your immediate experience, but the figure definitely has not the "*i*" quality. (As the effect of the Müller–Lyer device differs for various individuals it will be best if the reader draws a figure allowing for the effect in his own case.)

Presumably the "i" quality is to a considerable degree dependent upon the visual relationship between the visual direction of the lines and upon the seen position of the visual point *B* in relation to the visual line *DE*. (It would here have been more correct to speak of the aspects belonging to these factors, as it is most reasonable to regard the aspects and not the factors as causally connected). Whether we speak about aspects or factors our point of view here agrees with Professor Tranekjær Rasmussen's opinions which were put forward in § 10. In the present case, however, the matter can also be looked upon from my older point of view which is touched opon in § 9.

Looseness in the coupling shows itself by the fact that the Müller–Lyer device can alter the visual lengths of *DB* and *EB* in the part-figure 5 without simultaneously altering the character of the whole figure. It is remarkable that the looseness can show itself in a comparatively simple figure where there are relatively few factors or corresponding aspects.

§ 23. It would be of importance in connection with that which is touched upon in the latter sections to consider more closely to what the relationship between the deductive and the causal-empiric leads in actual physical figures in our surrounding physical world. Such a consideration must be irrespective of whether, for example, the figures consist of graphite on a piece of paper, of metal strips, or of a triangle consisting of imaginary lines between three physically marked points.

§ 24. Up to now in this paper we have, so to speak, miscredited mostly the visually experienced parallelism as the source of the difficulties. In these cases we could, perhaps, with nearly equal right have accused the seen length of a straight line of not having all the properties it should (if it is quite in order to take over the term "length of a line" from geometry). It was something like this which I had vaguely in mind in my old theory which I tried to make explicit in § 9.

At all events, there are figures where it seems natural to incriminate the visual length of a straight line.

Look at fig. 11. You may, as many people do, visually experience 1) a as larger than b (physically a is shorter than b) and 2) x as shorter than y (in accordance with the physically given). The line h is the perpendicular from the apex.

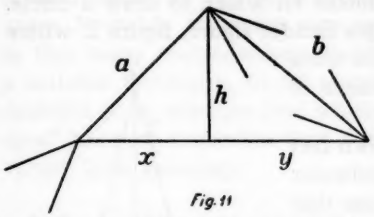

Fig. 11

This is easily seen to be incompatible with geometry. You can for instance from the two small right-angled triangles get, according to Pythagoras: $h^2 + x^2 = a^2$, $h^2 + y^2 = b^2$. From this follows, as $x < y$, that a should be shorter than b; but visually you have $a > b$.

Let us look at figure 12, which can be seen as being an isosceles triangle ABC where the angle B is bisected by BD. Here AD is seen to be longer than DC. It is difficult to say whether you see D as being in the middle of AC and the two half lines as unequal, or whether you experience D as not being in the middle of AC. Both experiences are contradictory to what can easily be deduced geometrically from the facts which you see, namely, that ABC is an isosceles triangle and that BC bisects the angle B. This deduction is so easy that you seem to see the result of it.

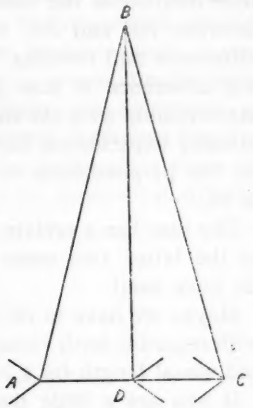

Fig. 12

A sort of modification of this figure is fig. 13, which was given to me by Mr. F. From, assistant at the laboratory. C is seen to be the centre of the circle, but the diameter AB is not seen as being bisected by C. Of course I think that my figure is the most pronounced, whereas Mr. From favours his.

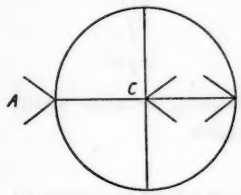

Fig. 13

If you want to have a curious experience, draw a Müller–Lyer figure, where the two lines a and b (fig. 1) *look* alike. Here you have

to make the part *a* physically somewhat larger than *b*. Use this figure as the baseline on which to draw an isosceles triangle free-hand.

You can make an analogous experiment, where you use the Müller–Lyer figure as a diameter on which to draw a circle.

We may return to the simple Sander figure, figure 2, where we can find a new, though for some not so very pronounced, instance of a contradiction.

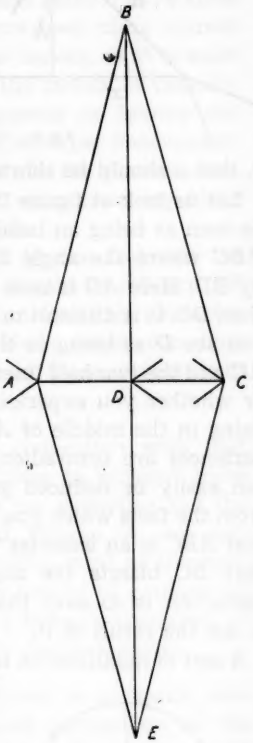

Fig. 14

You can emphasise the known fact that that *A* lies on the perpendicular to the middle of *BC*, and also see that this is so. This rather analytical attitude diminishes the visual difference between *AB* and *AC*, but a certain difference still remains. This remaining difference is also geometrically incompatible with the simultaneously visually experienced fact that *A* lies on the perpendicular to the middle of *BC*.

The case has a certain resemblance to the latter two cases with which we have dealt.

Maybe we have to revise our ideas with regard to both visual parallelism and visual length (of a line).

If you are a little trained in "introspection" you can shade the lower part of the triangle in figure 12 with your hand or something else. You can then compare what you see under this condition with what you see when you have the whole triangle before you. You will find that there is something peculiar about the whole lower right hand part of the figure.

§ 25. If you now look at figure 14, which is allied to figure 12 in an easily understood way, you will find that although both the triangles *ABC* and *AEC* are seen to be isosceles, and *BE* is

seen to bisect the angles B and E, the area to the right of BE is experienced as being much smaller than the area to the left. Probably this "compressing" of the one area was that which was foreshadowed in the lower right hand part of figure 12. In the present figure we can, so to speak, locate the contradiction in areas. Probably areas are very sensitive to "illusion devices" so that many contradictions could be demonstrated if we had a suitable technique. At all events, figure 13 alone makes it doubtful as to whether that which we call a "visual" or "seen area" has such properties that it is defensible to take the term "area" from geometry.

§ 26. In this paper, apart from where we mention an investigation by Professor Tranekjær Rasmussen (§ 10) we have only made use of an extremely simple technique and simple decisions from the subjects. It seems probable that if we used a much more refined technique with systematically built up experiments — as, for instance, when you determine a threshold — which could yield quantitatively graduated results, we would have an opportunity of finding many more cases of incompatibility with geometry than can be demonstrated with our present technique [6]).

[6]) Manuscript received 21 May, 1949.

PRODUCTIVE PROCESSES IN LEARNING AND THINKING *

BY

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"The formation of purpose, the vision of ends, and the subordination of the means to ends are the very essence of all liberties."

B. Malinowski, "Freedom and Civilization".

I. THE PROBLEM

In the modern psychology of thinking a distinction has sometimes been made between productive and re-productive thinking (6 and 9). This distinction rests upon the assumption that re-productive thinking constitutes the modification of an earlier problem solving or of other earlier behavior dependent upon the given individual's past experience. Consequently a certain continuity allegedly exists between re-productive thinking and the reactional autobiography of the individual organism.

In contrast to this it is assumed to be a characteristic of productive thinking that something is created that is *new, original,* and a first time occurrence within the individual's life. Apparently there exists *no continuity* between this new creation and the earlier experiences. The first solution of a novel problem is dependent upon the structure of the actual situation and not upon the past experience of the organism (3). Upon closer analysis it becomes evident, however, that the criterion of continuity presents a complex and not yet clarified problem as to the basic processes involved. The investigations by Katona (4) and Székely (10, 11) concerning memorizing and learning by understanding throw a completely different light upon the question of continuity. Katona had subjects learn the solutions

* Translated by A. Katzenstein and M. Scheerer, University of Kansas.

of match stick tasks by memorization while other subjects solved the identical tasks by insight. He then examined the effect of the respective learning experiences upon the solution of new problems. In principle the subjects who had learned by memorization were unable to solve new problems, and only able to solve the old tasks. The subjects who had learned by understanding were able to solve also new tasks, i.e. they were able to produce something original. Memorizing benefits only such subsequent situations which are identical or very similar to the original learning situation. The resulting behavior thus has the characteristics of reproductive thinking. Insightful learning, however, benefits also such subsequent situations which show a greater or lesser degree of dissimilarity to the original learning. The question might therefore be asked: "*Does a functional connection exist between insightful previous learning and subsequent productive problem solving?*"

Earlier studies of this author led to the conclusion that knowledge acquired in school may be utilized in different ways. Some subjects were able to apply their knowledge only to the solution of such tasks which required exclusively re-productive thinking (as for instance, the inclined plane task). Others were able to utilize their knowledge also in the solution of such problems which required productive thinking (as, for example, the solution of the hydrostatic-lever task) (12). The problem is therefore: *does insightful learning constitute the necessary condition for the utilization of what has been learned in the future process of productive problem solving*? Should it turn out that this question can be answered in the affirmative, then the functional continuity between productive thinking and earlier experiences will have been demonstrated. Then however, the second question arises: how is the impression created that there is no continuity between productive thinking and earlier experiences?

In a previous study it was shown that problem solving depends upon the structure of the actual problem situation (12). In the present study we will try to examine the question whether the solution of a reasoning problem is independent or dependent on the characteristics of an earlier experiential situation. In the former study the tasks were varied. In the present experiment the task remains constant and only the experiential situation (learning condition) is changed. We start out with the working

hypothesis that if the learning processes have any influence upon the course of later problem solving, then this should manifest itself in the fact that different learning processes have different effects upon the solution of an identical task. To verify this hypothesis, learning and problem solving experiments are combined. The learning material remains unchanged, only the conditions and method of learning are varied.

II. Procedure

1. A reasoning problem was chosen which the subjects, lacking relevant knowledge, would be unable to solve unless they had acquired the necessary information in a preceding learning period. The experiment had therefore two parts: a learning- and a problem-solving period. An interval of a few days separated the two. Care was also taken that the subjects did not suspect any connection between the learning and problem solving period. On the day of the training period, prior to the learning task proper, the S.'s were asked to solve a number of different reasoning problems and on the day before the critical reasoning experiment, they also received some reasoning problems of a different character. Thus they were led to conceive the two parts that actually belonged together as two tasks which were not connected. The S.'s were divided into two groups: in both groups the same material was learned, however, the method of learning was different. Since the one method corresponds closely to the traditional learning method used in European schools it will henceforth be called the *traditional method*, and the corresponding group of S.'s will be designated as the traditional group. The other method of learning corresponds to the method by which knowledge is acquired in a so-called modern "progressive school" (activity and work projects). This method will henceforth be called the *modern method* (the corresponding group of S.'s, the modern group).

The learning material, identical for both groups, consisted of four typewritten pages, containing a text on the elementary concepts of mechanics: work, output, propulsion, momentum of rotation, momentum of inertia, and impulse of rotation. Mathematical derivations and formulae were not included.

2. The learning instructions for the traditional group (after preceding reasoning problems) was:

"Now we come to a learning experiment. You are to study these four pages, and I will examine you in a few days to determine how much you have retained. The material consists of a few statements on mechanics. Some of it will be a refresher of previously acquired school knowledge, some will be new. You

do not have to learn it by heart. Your main task is to understand everything you read (this was strongly emphasized). In case something is not clear to you, say so immediately, I shall then explain it to you. During the examination later on you will *not* have to recite what you have learned verbatim or in connected discourse, but you will have to answer some questions on a questionaire in writing." Following the reading of the paragraph on momentum of rotation and momentum of inertia, some actual demonstrations were given. Momentum of inertia was visually concretized by means of a *torsion pendulum.*

The torsion pendulum: (Fig. 1) A bar of about 40 cm length

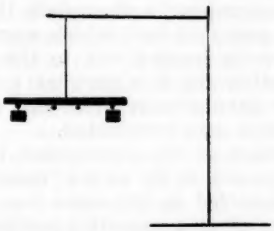


Fig. 1. Torsion-pendulum

is suspended on a thread in a horizontal position. Four hooks are attached onto the lower surface of the bar. Of these, two are symmetrically located near the point of suspension (inner hooks), and two others in equal symmetry located near the ends of the bar (outer hooks). Through repeated rotation of the bar a torsion tension (i.e., a momentum of rotation) is produced in the thread. Now, two brass weights of 54 grams each are suspended at the two inner hooks and the speed of rotation of the pendulum is observed. Then the weights are suspended at the outer hooks. The momentum of rotation of the thread has remained the same: however, owing to the change in distance of the weights from the axis of rotation, the momentum of inertia of the weights increases (and thereby, of the whole system) so that the speed of rotation is greatly diminished. The angle-speed relations are approximately 4 to 1. This demonstration is repeated twice with corresponding explanations and concludes the learning period.

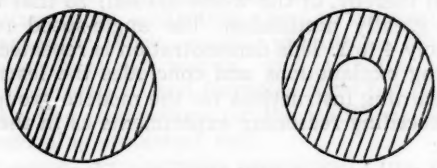
3. The learning instructions for the modern group (following the same preceding reasoning experiments as in the traditional group) was:

"Now you will begin a new problem." Thereupon the torsion pendulum is placed before the S.'s. By rotating the pendulum bar, a torsion tension is produced. While holding the pendulum in that position, the Exp. asks the S.'s: "The two weights can be attached on the bar symmetrically to its point of suspension,

either close to it or farther away from it. How, do you believe, will the position of the weights influence the speed of rotation if the rotating momentum (the force of rotation) remains unchanged?" As a rule the following answers were given: "I don't know", or I don't think so, because the weight is unchanged", Some S.'s conjectured that, "In peripheral positions of the weights, the speed of rotation would be greater because the centrifugal force would be greater." Then the Exp. carried out the actual demonstration which was generally a surprise for the S.'s. After this the Exp. stated: "You were unable to predict this effect, and even now cannot explain it, because you lack the necessary knowledge of physics. Please read these four pages. You will then be able to comprehend and explain the observed phenomenon." The four pages of text which were also used by the traditional group were handed out to the S.'s. After having completed the reading the S.'s gave as a rule a satisfactory explanation of the phenomenon. With this the learning period for the modern group was terminated.

4. The continuation of the experiment took place after an interval of 2—4 days and, as far as the "external conditions" are concerned, was conducted in the same way with both groups. At first the S.'s were given a reasoning problem of no connection to the experiment. After completion of this, followed the *problem of the two spheres.*

Instruction: "Imagine that we have two metallic spheres of exactly equal size and exactly equal weight before us. The surfaces are covered with a lacquer which makes it impossible to distinguish the spheres from one another. However, there exists a concealed difference between the two spheres. One consists of a very light, the other of a very heavy metal. How is this possible?" Most S.s were able to discover, after a short reflection, that the heavy metal sphere must be hollow, and the other solid. In case that a S. had been unable to arrive at this conclusion after a short time, the Exp. himself gave the explanation. Then the Exp. drew a cross-section of each sphere through its respective equator on a sheet of paper (Fig. 2).



Frg. 2. Problem of two spheres

He emphasized that the hollow space of the heavy-metal was exactly concentrical and now presented the problem proper: "How

could we determine which of the two spheres is the hollow and which the solid one? There are no physical or chemical tools at your disposal. The spheres are not to be damaged, no holes drilled, no pieces cut out, and no paint removed. Try to think of a method so simple that we can decide this question in this room without any apparatus. It is desirable that you think aloud, and even express such hunches, ideas, or doubts which you consider to be inappropriate. You may also ask me as many questions as you wish." (Solution of the task: If the spheres roll down an inclined plane or are set in motion on a horizontal plane with the same force, the speed of the hollow sphere is lower because its momentum of inertia is greater).

After conclusion of the task of two spheres, the S.'s of both groups received a questionaire with 11 questions. Some of these were memory questions on the learning material, while others were easy reasoning tasks which required less direct and specific utilization of the learned material. One question was, for instance, "What is momentum of inertia?", another was a reasoning problem such as "A man is standing on a swivel chair. The chair is rotated slowly. During this rotation, the man changes his posture — he extends his arm and one leg horizontally. What will result?" (Answer: Rotational angular speed decreases because the momentum of inertia of the body has increased).

5. Selection of subjects: For statistical evaluation only, the experimental records of those S.'s were used who had a Liberal Arts education. Not a single one of them had any information about the momentum of inertia previous to the experiment. According to the curriculum in the Swedish schools it was thereby insured that the knowledge relevant to the solution of the problem had been acquired under experimentally controled conditions. The experiments were conducted in the Institute for Psychology and Pedagogy of the University of Stockholm during the summer term of 1945. 40 members of the experimental psychology class volunteered as subjects. 20 were assigned to the traditional, 20 to the modern group. All subjects had previously served in another experiment. In that study (12) it was shown that the effect of acquired knowledge, as for instance, concerning the hydro-static principle, depends upon individual differences as to the degree of general application by the S.'s. According to that study the S.'s fell into two categories. In one, S.'s who in the early experiment on solving of the hydro-static-lever II problem (arrangement of metal weights) had shown a functionally effective knowledge of generalized applicability concerning the hydro-static principle. These were designated as a group with *generalized understanding*. In a second category those S.'s who, in solving the same problem had indicated less generalized application and depended more upon their specific concrete

experiences as point of departure, were designated as the S.'s with *less generalized* understanding. The S.'s for the present experiment were divided by these criteria of performance on the hydro-static-lever II problem into two groups: 20 with more, and 20 with less generalized understanding. They were then distributed into both learning groups in such a way that each learning group contained an equal number of subjects with generalized (10) and with less generalized understanding (10).

III. Results

As is evident from Table I, a clear difference exists between the results of the two groups, a difference which favors the modern method of learning. In the modern group 13 S.'s solved the task of the two spheres, and only 7 failed. In the traditional group, however, only 4 S.'s succeeded and 16 failed. The 40 S.'s produced a total of 17 solutions to which the modern group contributed 13, and the traditional group contributed 4. Correspondingly, of the 23 failures 7 occurred in the modern group

Tab. I.

	Solvet Tasks	Did Not Solve Tasks
Mod. Method of Learning N = 20	13	7
Tradit. Method of Learing N = 20	4	16
Total N = 40	17	23

and 16 in the traditional group. (The S. was regarded as having failed when in the course of "thinking aloud" he had thought of rolling or rotating the spheres and then had himself rejected the idea with the critical remark, "No, it wouldn't make any difference, because the spheres are of equal weight.")

Table 2 presents the solution frequency of the two spheres task in the different learning groups. It shows the relation of these frequencies to the degree of generalized understanding of the S.'s in the hydro-static-lever II problem. From the results the following seems evident:

1. The subjects with generalized understanding for the hydrostatic principle also did better with the problem of the two spheres than the other group did. Of the total of 17 solutions, 12 occurred in this group. This suggests the possibility that the

group is more endowed with generalizing capacity than the other.

2. The modern method of learning favored greater frequencies of successful solutions in both so-called "endowment" groups. We even find a distribution of solutions according to both the degree of endowment and the learning method. The best results are achieved by the 10 "more highly endowed" who have learned according to the modern method: 8 solutions against 2 failures.

Tab. II.

	N = 20 More Highly Endowed Group		N = 20 Less Highly Endowed Group	
Modern Method N = 40	Solved 8	Not Solved 2	Solved 5	Not Solved 5
Tradit. Method N = 20	4	6	0	10
Total N = 20	12	8	5	15

$\varrho = 0{,}66 \pm 0{,}18$

The most unfavorable results were obtained by the so-called "less endowed" who had learned according to the traditional method; all failures. The two other groups (10 more endowed in the traditional group and the 10 less endowed in the modern group) achieved approximately equally good results. The ten "less endowed" were able to apply their knowledge gained by the modern method of learning at least as productively as the "more highly endowed" who had learned by the traditional method.

3. The results further show: if different learning methods are employed the applicability of the same learned material, i.e., of the identical "material of knowledge" varies correspondingly during a subsequent productive solving process. *The problem solving process is not only dependent upon the actual situation, but also upon the characteristics of those processes which correspond to experienced situations in a (temporally) earlier phase of the individual's life history.*

The unequal effect of the different learning methods manifested itself not only in the differential frequency of solutions, but also in the introspective differences during the solving process.

The solving process: The S.'s were faced with a problem: there

are two spheres between which a definite, yet not perceivable, difference exists. One is hollow, the other is solid; I have to determine which one of the spheres is hollow, which solid. The S.'s *understood* the task in the following manner: I have to search for *another* difference, which can be directly determined and is connected with the first difference. This other difference should be an indication of hollowness or solidity. In order to produce this indication, something has to be done with the spheres: What could I do with the spheres in order to produce that indication? The S.'s thus approach the solution with a vague plan:

Pl^0 Find the difference....? What should I do....? The individuals had the feeling of groping their way into unknown territory, a vacuum, because, "I know too little physics and chemistry." The sought-after difference points already from the start in one or the other direction; usually certain subjects look for an indication by which to determine hollowness or solidity, others search for some indication of the quality of the material (eg. heavy, or light metal).

According to the above "plan" (Pl^0) the S.'s suggest different solutions: "To knock at the spheres and to utilize the *sound* difference." "To throw them into the water: does a hollow sphere float?".... "To heat them." Here the S.'s have no specific expectations as to what kind of differences would appear. Or, "Something with electricity." S.'s are here also lacking specific expectations as to the resulting difference.

Proposals of the latter kind were a clear indication that the S. did not rely upon his knowledge, but simply "guessed", and that he left it to his good fortune to hit accidentally upon the correct solution. Such proposals were usually rejected by the S.'s themselves in selfcriticisms. If they neglected to do so, the Exp. himself informed them that such a proposal was inappropriate, and why. After a few solution attempts of this kind the *modern* group showed the following rather typical thought-sequence in final solution: "How would it be if one would make the spheres roll (rotate) ....?....hm....yes. I have the *feeling* that the two spheres would roll at different speeds, that the hollow sphere would rotate more slowly." Exp.: "Why?" "I don't know and can't explain it, but I feel certain that the hollow sphere would move more slowly." Some S.'s call the "feeling of certainty" an "intuitive conviction", or "intuitive feeling",

"intuitive knowledge". Here, the Exp. steps in: "If you are right, there must be a reason for it; try to clarify this." While this was said the S. was exploring the diagramatic drawing of the hollow sphere. Now het exclaims: "Here, in the hollow sphere it is like that.... more on the outside...." The S. indicates with the pencil: "And here in the solid sphere, it is on the inside too. Yes, ...here the mass is more on the outside, peripherally concentrated, here the mass is also in the center. And the mass more to the outside constitutes a greater resistence to the rotation; therefore, it rolls at a slower speed."

The whole reasoning situation has now been changed. Actually the solution has been discovered. The question Pl^0 was followed by the answer: (A1) = "Make the spheres roll, the hollow sphere rolls more slowly." And immediately from this the new question arose, P2 = "Why should the hollow sphere roll more slowly?" The subject had a vague, unclear notion of the answer: "It must have something to do with the distribution of masses within the spheres." It has relation to the fact that the mass in the hollow sphere is further removed from the axis of rotation than that in the solid sphere. The S.'s had often great difficulty in finding the right words to express their thoughts. They busied themselves with the diagrams of the spheres, traced them with their fingers and accompanied them with such comments: "Here, it is like this.... further away.... here it is inside, too...." etc.

At this point, the S.'s were asked a check-up question: "In case the two spheres were placed in two equal little cars and rolled down an inclined plane, would there be any difference in speed?" In all cases the correct answer of "No" was given without any hesitation. The correct explanation could also be given: "The spheres do not rotate and their weight is equal." During the problem-solving — as well as after its termination — most S.'s of the *modern* group did not become conscious of the connection between the learning material (torsion pendulum task) and the problem of the two spheres. Neither did they spontaneously notice any analogy. They were not conscious of the fact that they applied the knowledge previously gained in the learning period to the solution of the present problem. Even when, after termination of the experiment, the S.'s were asked if they had noticed any connection between the present and any of the earlier tasks, most of them answered in the negative. Introspect-

ively, there was only the certainty that the hollow sphere would move more slowly, and gradually the insight developed that this had to do with the distribution of the masses. The S.'s could not indicate how and from what sources this „certainty" was derived. During the reasoning process only 5 out of the 13 successful S.'s noticed the connection of the present task with the previous learning material and made conscious use of it, e.g. drew a conclusion by analogy in considering the spheres analogous to the torsion pendulum.

The solution process took quite a different course in the *traditional* group. A solution arrived at by "intuition" or "feeling" never happened. The successful subjects however stated: "This must have some connection with what I learned last time.... yes, it has a connection with the momentum of inertia." Or, "I believe this is based upon the same principle as the experiment with the bar to which weights were alternately attached at the center and at the periphery." "Here in the hollow sphere" his reasoning continues, "the mass has also a greater distance from the center, therefore it will rotate more slowly."

The connection with the learning material was noticed by 6 unsuccessful S.'s. While trying to find the solution they commented on the analogy with the torsion pendulum, and they speculated on the difference in the momentum of inertia, but were unable to solve the problem.

Result of the examination concerning the "reproductive" knowledge: In the *modern* group only four S.'s were able to repeat verbatim the definition of momentum of inertia. But none of these was able to solve the problem of the spheres. In the *traditional* group, eight S.'s were able to repeat the definition correctly. Of these, only 2 solved the problem of the two spheres, one produced a part solution, and 5 failed. Under these experimental conditions of learning the *productive application and verbal reproduction,* i.e. *remembering of the learned material appears to be mutually exclusive.*

IV. Discussion

The thought that one could distinguish between the two spheres by rolling them, because a hollow sphere would move more slowly under equal conditions, was a completely new and original idea in the life of each of the subjects. None of them had read this idea or a similar one in a text or elsewhere. No

continuity existed between reproduction of this idea and the earlier experiences — qua molar events. (14) We must assume however, that there existed some connection between this reasoning and the acquisition of knowledge in the learning period. We must assume that during the learning period certain intermediate processes took place, and also, that with the different methods of learning these intermediary processes had different characteristics. The differences between the solution processes apparently depended upon the characteristics of these intermediate processes. *Qua molecular events there exists a connection between experience and productive thinking.* We may now attempt to construct a picture of the nature of these intermediate processes: about their rôle in the "terminal phase", the productive thought for solution as well as their rôle in the "initial phase", the learning period.

The solution was obtained by looking at the 2 spheres from a new aspect: "Unequal distribution of the mass". Having cognized the two spheres as possessing different distribution of mass, a new property can now be discovered: different speeds of rolling, i.e. of rotation. This change of aspect and the discovery of the new quality was dependent upon the experience that the subject had made in the learning period. Such a conection existed even in those cases in which the S. was not conscious of it.

In our environment, metal spheres are perceived as equipped with a set of habitually accepted properties such as weight, size, form, etc. In the attempt at solving the problem, these habitually accepted properties become first focal because the goal requires the finding of a difference between the 2 spheres. But in the area of the habitually accepted properties no difference could be found between the two spheres. Thus the S.'s left this habitual field of search "and ventured into the unknown": unknown thermal or electrical properties were looked for. During the process of "search" for a difference the S. discovered a new property; the distribution of the mass which is not one of the habitual qualities, was discovered and recognized as the cause of the difference in speed. In the process of reasoning, the conception of the sphere was enriched by a new property.

How is this process dependent upon those processes which have taken place during the learning period?

The property: "Unequal distribution of masses — unequal

speed of rotation" did not exist in the world of the S.'s prior to the learning period. That property did not exist after the learning period in this articulation. The test mentioned only the unequal momentum of inertia of one and the same particle of mass as a function of its distance from the axis. Neither did the S. gain any other information from the torsion pendulum than that changed position of two attached weights in relation to the axis changes the speed of rotation of the bar in a certain way. There is no indication for the assumption that already during the learning period the positional change of the weight was recognized as a change in the distribution of the mass. No actual change in the position of particles, of mass, or of weight, took place with presentation of the two spheres problem. It was a new process when the S. perceived the change in position of the weights of the torsion pendulum as a change in distribution of mass. The S. thereby discovered the dissimilar cross-section of the spheres, and different positions of the pendulum weights as situationally, i.e. functionally, equivalent. This new process did not take place in the process of learning, but only in the process of problem solving. On the other hand this restructuring was made possible by processes which had taken place during the learning period. In the problem solving situation the S. responded to certain properties of the new material which had a structural relationship to the processes in the learning period. He organized the structural relations which he had encountered during the learning period in a completely different situation into a new direction during problem solving. The introspectively described "intuitive" and "felt" solutions might serve as further clues for interpretation. We observed that the problem of the 2 spheres could be solved even when the acquired knowledge was not phenomenally given, i.e. consciously unavailable. The S. reacted in the problem situation to organizational factors which originally belonged to the learning situation, even if the latter failed to be represented in memory. The organizational factors relevant to the problem solving, though belonging to the earlier situation, emerged as belonging directly to the problem at hand, and were productively re-worked and further developed. It appeared as if this relational structural factor had been abstracted from the original learning objects, lifted from the context of the original situation and thus transferred to the actual thought-

object of the new task. Apparently this abstraction and achieved independence of the relevant structure was the prerequisite for the transposition to the new situation. Moreover, this process took place more readily if the original situation was not consciously represented in the actual process *). If the learning situation were thus present phenomenally (in consciousness) the first encountered structure of the original situation would remain "attached" to the original material as if "fixated", and would be more difficult to transfer. Thus we arrived at the conclusion that it depends upon the conditions of the learning process whether a later transfer of the learned structural relations will occur more or less readily.

In earlier studies (10, 11, 12) a theory concerning the difference between memorizing and learning by insight has been advanced. According to this hypothesis, in the process of insight-learning, an item of present thought contents, (i.e. an item previously acquired, a naive experience with something) was reorganized. In memorizing, however, no change of such acquired knowledge has to take place. Productive thinking and insightful learning have therefore in common the fact that creative reorganization takes place. The results of the present experiment confirm this hypothesis. The question of the temporal localization of production processes might therefore be answered by concluding that such *creative processes of reorganization take place in learning as well as in reasoning.*

V. Outlook towards new problems

The essential fact of the productive process in both is that the S. grasps the structural relation in terms of principles of organization which transcend the individual specific properties of the material. The S.'s who have proved to be more easily capable of such generalized understanding in earlier experiments also succeeded better in the present experiment, even when placed in the traditional learning group.

The present study confirms the hypothesis made in earlier works that the productive application of knowledge depends

*) ".. to drop the idea once it has been formulated, to forget it.... Then when we unexpectedly come upon the idea again, it will often have developed, will show new aspects that we had not seen formerly." Th. Reik: Listening with the Third Ear. Farrar, Strauss & Co, New York, 1949, p. 209.

upon the manner in which the material was mentally processed during learning. In the present discussion understanding was equated with productive processing. Understanding, however, is an expression designating phenomenal facts. Understanding is an experience (conscious). Productive processing, restructuring, etc., however, are not necessarily phenomenal facts. People, for instance, rarely experience that, when learning the hydrostatic law, their naive knowledge by acquaintance is restructured; furthermore, no. S. in the *traditional* group had had the experience of not comprehending the learning material. To a certain degree they also had understood the material which they had learned. If they had not, they would have been unable to answer the reasoning question which they were asked on the questionaire. The *experience* of understanding is therefore neither a reliable reflection of occurring processes nor an essential component. Introspective self-awareness captures very inexactly what actually happened. Consequently, "understanding" or "insight" as used throughout this paper are to designate processes and not experiences.

Our experiments have shown that the modern method of learning offers *greater chances* for the occurence of productive processes than the traditional method. We do not know upon what factors this depends. We do not even know if it depends exclusively upon such factors which are inherent in the method of learning, or whether other factors (i.e. factors introduced by the experimental technique) have contributed to the final result. Therefore, further studies are necessary. The following problems arise: 1. the factor of *completion-incompletion.* When the S.'s in the *modern* learning group are given the problem of the two spheres, the learning presents a task completed long ago. The S.'s of the *traditional* group however expected an examination. Therefore the S.'s learning period has not been completed. Perhaps their more frequent recall of the learned material during the subsequent problem solving and test-like exam was caused by this factor (7, 15). What would have been the proportion of frequency of recall in both groups if, at the time of the problem solving, both groups would have experienced the preceding learning period equally as either completed or uncompleted tasks? Is there a relationship between the tendency to recall material in unmodified form and the difficulty to apply it in the

structurally abstracted form during productive problem solving?

2. *Factor of isolability by virtue of non-homogeneous field.* In the modern method of learning, the task of the torsion pendulum was one among several other tasks representing reasoning problems. This factor of homogeneity favors the integration of its corresponding trace system "in loco" and hindered its reproductibility (8), in unchanged form. In the *traditional* learning method, however, the learning period constituted a relatively heterogeneous task in a series of problem-solving tasks. This factor of heterogeneity favored the isolability of its corresponding trace-system as a relatively independent, individualized, and coherent organization. This factor of greater isolability could have its effect in the same direction as the factor 1 of incompletion. Factors 1 and 2 were not inherent in the learning methods but were artificial circumstances introduced by the technique of the experimental set-up itself.

3. *A (hypothetical) quantitative or energetic factor:* A problem is more interesting than a learning task. Perhaps in solving the problem of torsion pendulum in the learning period the *modern* group invested more energy than did the *traditional* group during the learning of its explanation. It may be that this was responsible for more thorough assimilation in the *modern* group, and the inferior reproducibility.

4. *Factor of the attitudes or set:* In the *modern* learning method the S. had the same attitude or set in the learning and in the problem solving: namely that of problem solving. In the *traditional* learning method the attitudes or sets were different in both situations. In case this factor should have a differential effect, it could be interpreted according to Koffka's theory of the double memory trace (5) as well as according to Bartlett's conception of the importance of the attitudes (1).

VI. Pedagogical outlook

If we expect of school and learning the enrichment of our functional knowledge, if the school is to give us an education which furthers creative and critical thinking, then the results of the teaching which is done in the schools of Europe and even in the most advanced and democratic countries appears rather meager. The fine program: "Non scholae sed vitae discimus" remains a pious dream. In reality sterile memorizing is practiced.

The teacher controls "Knowledge" by requiring proof of ability to recall and believes that those who are able to "reproduce" learned material well will also be able to apply such knowledge creatively. This assumption is apparently false.

Summary

This study is concerned with the problem of the psychology of thinking which is relevant to practical education as well as to theoretical psychology. From a practical point of view the problem may be stated thus: "It is well-known that different individuals who have the same knowledge show great differences in the original application of this knowledge. Does the applicability of the knowledge depend solely upon the differences in individual endowment or is it also dependent upon the *method* of learning and teaching? Is it possible to increase the productive applicability of the knowledge that is the capacity for creative thinking by an improvement of the *methods* of learning?

The theoretical aspect of the same problem may be presented in the following way: In present day psychology of thinking, productive and reproductive thinking are regarded separately. The criterion of reproductive thinking is that the present problem solution constitutes only a more or less intense modification of an earlier experience, of an earlier problem solution. Productive thinking is judged to exist, if the present problem-solution constitutes an individual new creation which is not similar to earlier problem solutions of the individual nor to his previous experience. A certain continuity exists consequently between the reproductive problem solving and the life history of the individual, a continuity which apparently is lacking in productive problem solving. If we take into consideration, however, that even in the most original problem solving knowledge is applied which has been gained in earlier life, it becomes at once clear that the problem of continuity has to be formulated in a new way.

We distinguish, as does Tolman (14), between molar and molecular events. (In modern natural science one distinguishes analogously macro- and micro-events). The problem of continuity as it was posed up to the present was formulated in

molar terms and therefore, in those of everyday experience: i.e. of individual differences in the application of identical knowledge. Translated into molecular terms this everyday experience may be relegated to an implicit assumption about the temporal localization of the productive processes. The productive processes on the molecular level, which are assigned to correspond to creative thinking, take place only during the activity of problem-solving and not during the activity of learning or experiencing. Earlier studies on learning by insight (as contrasted with rote learning) however, make us suppose that these productive processes are also co-ordinated with learning by insight. In the present experimental study the correctness of this supposition is examined, and the possibility is explored whether there is a connection between the temporally-separated events of previous learning and subsequent problem-solving. The experiments are divided into a learning and a problem-solving period. The method of learning is variable, the reasoning problem remains constant. One method of learning is the *modern*, the other the *traditional* one. As learning material, a few laws of mechanics serve. As reasoning problem, a relatively difficult task, the problem of the two spheres, is given; this can be solved only by means of one of the learned laws.

Results: 1. The frequency of successful solution was considerably higher among the S.'s who had learned the concepts according to the so-called *modern* learning method than among those who had learned according to the *traditional* way.

2. The frequency of successful solution was also dependent upon the "endowment" of the S.'s. The *modern* method of learning increased the frequency of successful problem solving in both groups. The higher endowed solved the problem as frequently with preceding learning by the *traditional* method as the less highly endowed with preceding learning by the *modern* method. The less highly endowed who had learned according to the *traditional* method were practically *never* able to solve the problem.

3. The verbal reproduction of the learned material (laws of mechanics) stood in negative reciprocal relation to the frequency of successful solutions. S.'s who had learned by the traditional

method were able to recall these laws also, i..e. verbalized more frequently than S.'s who had learned by the *modern* method.

4. Productive application of the learned material, in the process of problem solving, and verbal reproducibility of the material, are apparently mutually exclusive (under the existing experimental conditions). Of the S.'s who were able to solve the problem, only very few were able to reproduce the definition. Conversely, of the S.'s who were able to repeat the definition of the concept verbally, only a few were able to solve the problem.

5. The course of the solution process proper showed outstanding differences between the two learning groups. S.'s who had learned according to the *modern* method of learning did not remember the learning material during the problem-solving. Even later most of them did not become conscious of the connection between the present problem-solving and their earlier learning experience. These S.'s designated the solution as achieved through "feeling" or "intuition". The functionally effective knowledge, i.e. the effect of pertinent past experince, was phenomenally absent during the period of problem-solving. This was different with the *traditional* learning group. These S.'s discovered that they were to use an earlier learned concept. They realized that the solution had an analogy with the torsion pendulum. They succeeded in reproducing the concept and yet were frequently unsuccessful in solving the problem.

6. The earlier assumption about the nature of understanding or learning by insight may be regarded as empirically confirmed.

7. Concerning the temporal localization of the productive processes, the following tentative conclusions were reached: Productive prosesses (organization, structuring, re-organization, and re-structuring) took place during learning by insight as well as during problem-solving proper.

8. Regarding the question of continuity, we concluded: if we observe the molecular event in productive thinking we are able to determine a continuity between the creative behavior and the earlier experiences of the organism. The course of present problem-solving activity is dependent upon the characteristics of such processes which have taken place in an earlier phase of the individual development.

9. Practical-pedagogical Application: The comprehension of learning material may be furthered by an improvement of the learning method. Through this, independent, productive thinking may be fostered.

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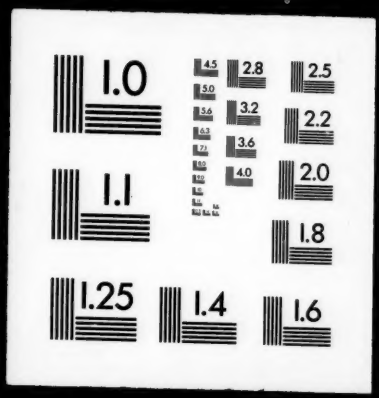
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